LONDON ELECTRICAL SOCIETY

LONDON ELECTRICAL SOCIETY.

At a General Meeting of the London Electrical Society, held in the Theatre of the Gallery of Practical Science, Adelaide Street, on Saturday, the 7th of October, 1837,

JOHN P. GASSIOT, Esq. Treasurer, in the Chair,

The following Rules and Regulations having been submitted by the Committee were unanimously approved and ordered to be printed.

Rules.

1. The Society to be called the London Electrical Society, and to consist for the present of resident and non-resident members.

2. All subscriptions to commence on the 16th of May, being the anniversary of the formation of the Society.—The subscription of a resident member, (or one residing within 20 miles of London,) to be for the first year, £2. 2*s*. and that of a non-resident, £1. 1*s*.

3. Visitors to the Evening Meetings may be introduced by any of the members and will be permitted to join in the discussions.

4. When one hundred names have been registered as resident members, the Committee are to take immediate measures to prepare standing Rules and Regulations, the same to be submitted at a General Meeting to be then especially convened for that purpose, when such proposed Rules and Regulations may be confirmed or amended, and the Officers and new Committee of Management appointed.

5. The Society to meet on the first and third Saturday of every month, at 7 o'Clock in the Evening, for the purpose of conversation; at 8 o'Clock a Chairman will be elected by the members then present, and the regular business of the evening will immediately commence.

6. At the first meeting of every month, and after the immediate business of the evening is disposed of, it shall be competent for any member to propose such a motion as he may consider advisable for the interests of the society, notice of which, however, must have been entered in the minutes of a previous meeting.

Committee.

The Committee, as at present appointed, is considered merely provisional. Its duties, until one hundred resident members are registered, (in conformity to the 4th rule) are defined as follows.—

The Committee will regulate the general business of the Society, and particularly the routine and proceedings of the evening meetings.

The Committee will receive and examine all papers or communications that may be tendered to the Society for reading at the evening meetings; but should any papers be received which the Committee should not consider applicable to the purposes for which the Society was established, such paper is to be returned by the Secretary to the Author: it being expressly understood that all papers retained by the Committee are to be read at the first opportunity which may offer at the evening meetings.

The Committee are (for the present) to elect all members; and any gentlemen wishing to join the Society will please forward his name and address to the Secretary, stating whether he is personally known to any member. When the Society has completed the number of one hundred resident members, the elections will afterwards take place in such a manner as the Society may then direct.

The Committee have the power of making by-laws for their own regulation; but any alterations in the general rules of the Society can only be made agreeably to the 6th rule.

The Committee may appoint sub-committees, to which any member, whether on the Committee or not, shall be eligible, for the purpose of examining or testing the experiments which may have been submitted to the Society.

As all papers or communications to the Society will be submitted to the Committee, it is understood that the Committee are to take charge of all papers retained by them; such papers to be deemed the property of the Society, unless any engagement to the contrary shall have been stipulated for by the author. They will also take charge of all books or apparatus presented; a notice of which they will instruct the Secretary to enter on the minutes of the next meeting after they have been received, when they will be duly acknowledged.

The Committee will direct the Secretary to register the names of all the members as they are elected, distinguishing whether resident or non-resident. Such register to be open to the inspection of any member, on application to the Committee.

Secretary.

The duty of the Secretary will be to keep the minutes of the meetings and to enter them in a book, to which any member may have access on application; to receive all letters and communications addressed to the Society, and to lay the same before the Committee at the first meeting subsequent to the receipt of such communication; to conduct the correspondence of the Society in such a manner as may be directed by the Committee; to edit the transactions of the Society; and to superintend the printing and the making of the index to each volume.

Treasurer.

The duty of the Treasurer will be to keep the accounts of the Society; to receive the subscriptions, and to enter the same in a book, which will be open to the inspection of the members; to present all accounts to the Committee, and, when audited, to pay the same.

Members.

When a member has been elected by the Committee, notice of such election shall be sent to him by the Secretary, and on his paying the amount of his subscription to the Treasurer, his name shall be registered by the Secretary.

Every member whose name has been registered has the right of being present at all general meetings; to introduce visitors either personally or by tickets under such regulations as the Committee may from time to time regulate.—He is entitled to join in the discussions, and at the first meeting of every month to propose any motion (under the regulation of 6th rule) he may consider advisable for the interests of the Society.

Order of Business for the Evening Meetings.

Immediately on the chair being taken the minutes of the previous meeting are to be read, and the names of such members as may have been subsequently registered announced. The papers that have been presented to the Society and accepted by the Committee shall then be read.—After each paper a discussion may take place, should the members present consider it expedient; but no new paper is to be read after 9 o'Clock.

WILLIAM LEITHEAD,

Secretary.

LONDON ELECTRICAL SOCIETY.

Address, delivered by W. Sturgeon, Esq., at a general meeting of the London Electrical Society, held in the Theatre of the Gallery for the illustration of Practical Science, Adelaide Street, on Saturday, August 7th, 1837.

Gentlemen,

The Report of your Committee having been read by the Secretary this evening, I am now called upon to explain to you the nature of this Society, and the objects for which it has been formed. I cannot proceed to address you, however, without, in the first place, expressing my entire concurrence in every particular embraced in the Report, with the solitary exception of the appointment of myself for the discharge of this important duty, being perfectly convinced that some other member, more efficient than I am, might certainly have been named. The choice, I am persuaded, cannot be thought to have arisen from any consideration of ability: I am more willing to believe that it is an act of polite courtesy on the part of your Committee, occasioned, perhaps, by the circumstance of my taking the chair at the meeting of a few friends to science, when the Electrical Society was first formed. But it will be remembered, by those gentlemen who were present on that occasion, that, notwithstanding the impressions which I experienced of the honour they were pleased to confer upon me, it was with extreme reluctance that I assumed an office, to fulfil the duties of which, others about me were more amply qualified. But the laudable motives from which this Society was originally instituted must ever claim the respect and regard of every friend to science; and the desire which I have, as an individual member, of being instrumental in promoting its present interests, and future prosperity, will, at all times, place my humble services at the command of your Committee, in any way they may deem them most useful; and I should wish it to be distinctly understood, by this assembly, that it is from a full conviction that the cultivation of electricity will ultimately confer the most important benefits on mankind, and from a firm belief that its data will become more numerous and exact, and consequently, its advancement more rapid, by a co-operation of experimentalists, than by the insulated position in which they have hitherto been permitted to labour, that I now appear before you as an advocate for the interests and success of the *London Electrical Society*.

From a consideration that the greater number of this assembly are probably acquainted with the progress which electricity has hitherto made, it is not my intention to attempt, in this address, to pourtray even a brief outline of the science; notwithstanding, I am inclined to believe, there are many who might suppose that, on an occasion like the present, a recital of some of the most prominent discoveries and events which have marked the progress of electricity, could neither be conveniently dispensed with, nor with propriety omitted. I am well aware, however, that the cultivators of electricity, who now hear me, have no need of my services to bring to their recollection the splendid discoveries which are recorded in the history of the science. Those are events already indelibly stamped on their minds; they are stimulants to renewed experimental enterprise, and are land-marks by which they are guided in the pursuit.

But it may be said, if those distinguished events which have marked the respective eras in the progress of the science be looked upon as shining beacons to the experienced electrician, it is possible, that even a brief view of their splendour might incite the amateur to renewed exertions, and lead him to discoveries still more important than any that have hitherto been recorded. Moreover, I should labour under no unpleasant apprehensions of being tedious to our veteran electricians, by a brief retrospection of some of the principal discoveries which have been made in their favourite science; because, I can easily imagine, that their ardour for fresh enquiries becomes rekindled, and reverberates with new energy, at every recital of those eventful periods, which are so prominently arranged in the same path of discovery, which they themselves are now successfully pursuing. But, however pleasing and useful might be the recital, I forbear detaining you with a detail of facts so abundantly recorded in almost every treatise on electricity, and so easily accessible to any one wishing to become acquainted with them.

No one, I am persuaded, can contemplate the multitude of facts which have been developed by the industry of experimentalists; the splendour and beauty of those facts; the simplicity of their production; the harmony of their display; their analogy to some of the most mysterious operations of nature; and their general tendency to improve the condition of mankind, and inspire the most sublime ideas that the mind is susceptible of enjoying; without entertaining the most flattering hopes of the success of a well conducted Electrical Society. Indeed, whoever takes a general survey of electricity, with its thousands of imposing phenomena, ramifying through an almost endless variety of nature's productions, and forming the most important experimental science ever cultivated by man, can feel no hesitation in acknowledging the probable utility, at least, if not the absolute indispensability, of a society wholly devoted to its cultivation; and it is hardly possible that he can quit the contemplation, without experiencing feelings of astonishment and regret that such a society had not been formed and matured many years ago.

The last forty years have been more productive of electrical discovery than all the previous centuries embraced in the history of the science; yet even seventy years ago, when electricity had not assumed half the importance it now presents, the eminent Priestly saw the necessity of Electrical Societies, as will be understood by the following passage from the preface to his History of Electricity. "The business of philosophy is so multiplied, that all the books of general philosophical transactions cannot be purchased by many persons, or read by any person. It is high time to subdivide the business, that every man may have an opportunity of seeing every thing that relates to his own

favourite pursuit; and all the various branches of philosophy would find their account in this amicable separation. Let the youngest daughter of the sciences set the example to the rest, and show that she thinks herself considerable enough to make her appearance in the world without the company of her sisters."

These, gentlemen, are the words of the first electrician of the age, written in the year 1767; long before Galvanism, Electro-magnetism, Thermo-electricity, or Magnetic-electricity were known. If, at that time, Electricity, the then youngest daughter of science, was considered of sufficient importance to make her appearance in the world alone, what would the learned Priestly have thought had he lived to see her make such progress as she has since done; to see her *not* the youngest daughter of science, but a parent of other sciences: and still without being deemed worthy of a separate establishment amongst their temples?

When speaking of incorporated societies, Dr. Priestly says, " I by no means disapprove of large, general, and incorporated societies. They have their peculiar uses; but we see by experience that they are apt to grow too large, and their forms are too slow for the despatch of the minutiae of business in the present state of philosophy."

This passage probably alludes to the Royal Society; a society for which I shall ever entertain the highest degree of respect, and which will, I hope, always maintain the elevated rank it has hitherto held in this, and, I believe, in every other civilized country.

Any one acquainted with the rise and progress of institutions must be very well aware that, however formidable or imposing may be their present aspect, most philosophical societies, of any note, have commenced under very humble circumstances, generally, by the uniting of a few friends who were favourable to scientific pursuits. It is well known, for instance, that the Royal Society emanated from the praiseworthy exertions of a few scientific individuals during the time of the civil wars. Its members rapidly increased in number, and no society has contributed more to the advancement of natural and experimental knowledge, than the Royal Society has done, by the publication of its memoirs, under the title of *Philosophical Transactions.* But it must be acknowledged, that a work embracing such a diversity of subjects, as, from the nature and constitution of the Royal Society, are necessarily printed in its transactions, is very far from being well adapted for the perusal of those experimentalists, whose enquiries are directed to one particular branch of science. The same remarks will apply to the transactions of every society whose object it is to promote, indiscriminately, the advancement of all branches of natural knowledge, and they are equally applicable to every periodical whose contents are of a miscellaneous character.

It is true the philosophical transactions are enriched by communications from men distinguished in almost every department of science; and, from the variety of topics they embrace, may be consulted with nearly equal advantage by the astronomer, the geometer, the natural historian, the anatomist, the physiologist, the chemist, and the electrician; but the astronomer has seldom a desire to be impeded in his favourite pursuit by having to traverse the memoirs of the chemist, or the electrician; nor has the physiologist any particular wish to read, much *less* to purchase, the works of either of them. It is thus that valuable papers, on particular subjects, are entirely lost, to those who would take the greatest interest in perusing them; and facts, which are

now re-appearing as new discoveries, have long lain smothering amongst heaps of records on topics entirely foreign to them.

Sensible of these impediments to the progress of their individual sciences, the Astronomers, Geologists, Horticulturists, Engineers, Entomologists, and some other classes of scientific men, have found it advisable to form themselves into distinct societies for the propagation of their respective branches of research. And it is really surprising that electricians, the importance of whose labours is second to none, should so long have permitted themselves to remain disunited and insulated from each other. Electricity, however, has hitherto been left to struggle through the exertions of individuals, many of whom, having fettered themselves with certain hypotheses, appear more intent on advocating particular theories than in furthering the cause of the science.

It is possible, I imagine, that some may be of opinion that, as the present age has produced so many experimentalists of acknowledged ability, electricity may be safely left in their hands. I am myself of that opinion; and it is one of the principal objects of this Society to place it in their hands *unitedly,* and as a body of electricians. The Committee, therefore, invite every electrician to join this Society, being confident that, by their united labours, electricity will speedily be placed on the same footing as the most acknowledged exact science. But, if the cultivation of electricity were to be still left to the caprice of individuals, it is not likely that their theoretical opinions would soon be reconciled to each other. Many of them would be still left to flutter on the wings of vain hope, and ponder away their valuable time in whimsical hypotheses which have no reality in nature. It would be easy to swell out this address with the hypothetical incongruities of the present day; but I forbear. Their only use is to show how cautious we ought to be in receiving as axioms the opinions of individuals, however eminent they may be esteemed for their scientific attainments.

Electricity is yet, and must for some time remain, a science of experiment and observation; and the advice of Dr. Franklin, to one who asked him for his opinion as to some hypothesis which he was anxious to advocate, cannot be too literally followed. "I would recommend you," said the philosopher, "to employ your time rather in making experiments, than in making hypotheses and forming imaginary systems, which we are all apt to please ourselves with until some experiment comes and unluckily destroys all our expectations."

The importance of Electricity has invariably been a subject of much comment at the annual meetings of the British Association; and many valuable papers, on certain branches of electrical science, have appeared in the volumes of its transactions: but it has not unfrequently happened that matter of the greatest importance to the general interests of the science, which has been elicited at the discussions, has been suffered to be entirely lost, excepting to those few who had the good fortune to be present. Every one who has attended the meetings must acknowledge this fact. Indeed, even those persons who have been fortunate enough to get their papers read, or apparatus exhibited, have usually been so hurried, from a want of sufficient time being allowed, to enable them to proceed in the calm uninterrupted manner essential to give full effect to their performances, that the principal part of the object for bringing them forward has been entirely defeated; while important information on certain points, which might probably have been developed by discussion, has been absolutely stifled, by the pressure of other matter, possessing, perhaps, equal claim to attention, at the same physical section.

It must, however, be acknowledged, that much praise is due to the projectors of the British Association. Its migratory meetings are well calculated to awaken the latent faculties of the thousands who attend them, and to inspire a spirit of scientific emulation throughout every province in the land. But its business is multifarious; and its transactions, which appear annually, are necessarily of a miscellaneous character. Moreover, much that is valuable, which transpires at the discussions, never appears in them; and which, if ever published at all, is by means of other journals. But I shall not dwell any longer on the proceedings of other societies, each of which has its particular uses, and is productive of more or less good to the general interests and welfare of mankind. I have mentioned these for the purpose of showing that their province is to propagate natural knowledge generally, and that they are not devoted to the culture of any particular branch.

This society has been formed for the purpose of offering to the cultivators of electrical science an opportunity of avoiding the various impediments which have hitherto been placed in the way of their own particular study. The Committee will afford every facility for the publication of their discoveries, as well as to their descriptions of new instruments, in the volumes of the transactions of the Society, which will be illustrated with appropriate diagrams, and devoted to no other subject than the various branches of Electricity. By thus encouraging the cultivation of the science, the Committee feel an entire persuasion that the number of members forming this society will speedily increase, and the progress of Electricity will thereby be rapidly promoted.

It has been stated, and I am aware with some truth, that, hitherto, the management of the different philosophical societies of London has generally fallen on the same individuals. That the prominent and most efficient members of the Royal Society, are also those of the Astronomical, Geological, and other Societies, and that it is impossible these gentlemen can devote any more of their valuable time to the services of any other, however important its object, or however much it may be required.

There is no one would more deeply regret than myself our being deprived of the co-operation of such distinguished individuals as now adorn the councils of those societies; and it is with a view of securing to the Electrical Society the advantages derivable from the services of those eminent men, that the Committee have advised the number of resident members to be augmented to one hundred before its officers are elected; and, sooner than the Society should lose any opportunity of securing the assistance of such eminent talent, I should strongly recommend even that number to be increased before its council is formed. But should we even not be fortunate enough to enrol such names in the lists of our council, there can be no fear as to the general result. The number of individuals previously unknown in the annals of science, who have, within the last few years, devoted their time as well as pecuniary means to the cultivation of electricity, affords this Society every prospect of receiving their assistance; whilst, on its part, it will become a parent, to foster and cherish their investigations; a grand storehouse, in which they may repose the rich productions of their labours, and a temple for their kindred spirits' resort.

I am well aware, that many are of opinion, that those alone who are deeply skilled in experimental investigation can really be useful to science. Such an idea is as groundless as it is detrimental to the progress of any particular branch. Let no one imagine that he cannot render science a service. I have already stated that electricity is a science of experiment and observation; and, therefore, this

Society cannot receive greater assistance than by the communication of such facts as, from time to time, come under the notice of its members. The Secretary will, at all times, be happy to receive such communications, and lay the same before the Committee; and should any member require information as to the proceedings of the Society, he has only to address a letter to the Secretary, who will immediately attend to his request.

These facilities, gentlemen, will, I hope, be an inducement for our amateur experimentalists to join the Electrical Society, by whose fostering care, and their own perseverance, they will gradually acquire dexterity and confidence in their performances, and will eventually become distinguished and valuable electricians.

There is another circumstance, to which I must allude, before I close my observations. It is intended, by the Committee, that the surplus of the income derived from the subscriptions over the current expenditure, will be principally devoted to the publication of our transactions. This will give every member, whether present or not at our evening meetings, an opportunity of knowing the progress, which the Society is making.

Before concluding this address, it perhaps may be necessary to allude to the circumstances from which this Society originated, as well as to the nature of the meetings which have hitherto been held under its auspices. In the spring of the present year, I delivered a course of lectures on electro-magnetism, and magnetic electricity, at Mr. Clarke's, Philosophical Instrument Maker, Lowther Arcade. At the close of each evening's lecture, a conversation generally ensued among the gentlemen who honoured me with their attendance; and the want of a Society for the encouragement of electrical pursuits was occasionally spoken of, and universally acknowledged. On the 16th of May, a few of those gentlemen met; and after some discussion as to the best mode of establishing such a Society, it was agreed that the attempt should be made. The first of our meetings took place on the 10th June, and they were continued each succeeding Saturday, until the 12th of August, the number of members gradually increasing the whole of the time. On each evening one or more papers were read, and several animated discussions took place, visitors being allowed, by the rules of the Society, to take a part.

Scarcely a meeting has passed without some new apparatus being produced on the tables; and I can, without hesitation, refer to the members, as well as to the visitors, for their approval of the order and regularity of our proceedings. The Society soon found it indispensable that they should have more accommodation; and the liberality of the council of the institution, under whose roof we are now assembled, has afforded us an opportunity of carrying out the original resolutions, even to a greater extent than we had anticipated.

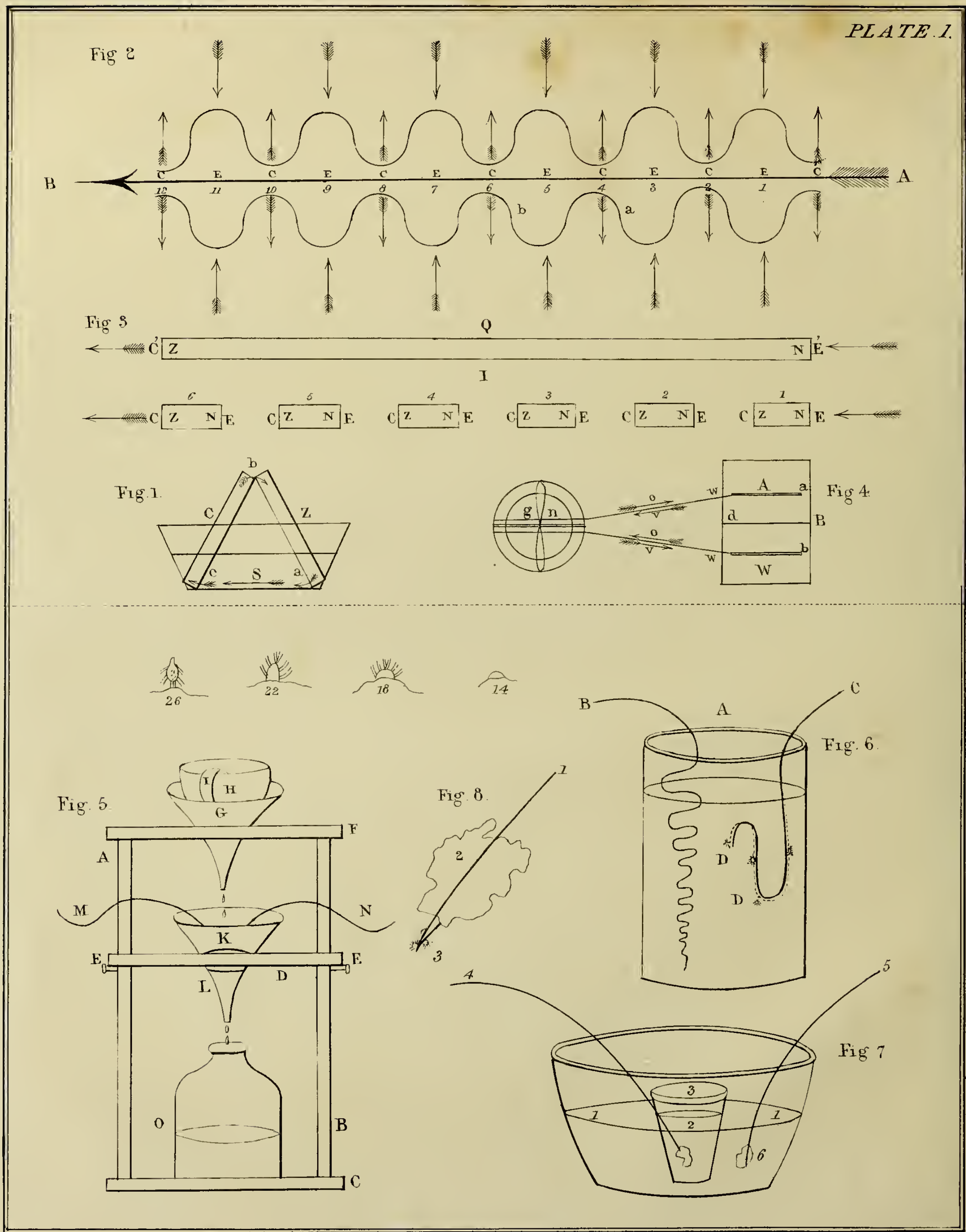
In addition to the Theatre, the council have offered us the use of the apparatus belonging to the establishment; and I look forward, with confidence, that the close of the present session will find us sufficiently increased in number to enable the Committee to state in their report, that the Society is sufficiently matured to have its officers elected.

In conclusion, I may state that the Society has already secured the services of an able and efficient secretary, and of a Committee whose members are indefatigable in their exertions, and in

whose hands the interests of the Society, may, with confidence, be reposed, until its numbers be sufficiently augmented to form a permanent council and appoint its officers.

From the attention with which you have honoured me, gentlemen, whilst delivering this address, and the repeated demonstrations of approbation which you have so kindly manifested on the various topics which I have noticed, I cannot but flatter myself that, notwithstanding my inability to do justice to the subject I have ventured to discuss, I have succeeded in convincing you of the necessity for, as well as the prospects of, the London Electrical Society; and I have no doubt that as soon as the objects of this Society become more generally known, its utility will be proportionately appreciated; and its interests permanently secured, by the number and ability of its members, whose talents will soon be united in rendering it support.

LONDON ELECTRICAL SOCIETY.



W Annan lith: 12 Gracechurch St.

LONDON ELECTRICAL SOCIETY.

I. *The Action of the Voltaic Battery shown to be two-fold, and the distinction between the terms Quantity and Intensity determined by the theory of vibration; with a reply to the various objections made to the theory. By* Mr. Thomas Pollock.

Read 21st of October, and 4th of November, 1837.

As the subject of this paper involves a general and important law, and may have a considerable influence in simplifying the theory of voltaic action, the investigations I am about to present to the notice of the Society will not, it is hoped, be unworthy of consideration. If it can be shown that the action of the battery is twofold; first in abstracting the electric fluid from bodies, and secondly in imparting it to others, an important point will be gained, and the progress of electrical science, in a steady course, amidst the conflicting opinions of the present day, may be anticipated. When we are informed, although on the very highest authority, that an atom of water yields as much electricity during its decomposition as is contained in a flash of lightning, we may well be startled with the assertion; but when we know that the power decomposing the atom of water arises from the abstraction of the electric fluid, and that the flash is an effect of the communication of the fluid, our astonishment ceases.

This subject resolves itself into four parts.—I. The alteration of form which the elements of the battery undergo. II. The explanation of these changes on the theory of vibrations. III. The distinction between the terms quantity and intensity determined by the theory of vibration. IV. The various objections made to the foregoing theory.

I. On the alteration of form which the elements of the battery undergo.

No attempt appears to have been yet made to exhibit the connexion that exists between the alteration of form in the elements of the voltaic battery, and its electrical properties. As far, however, as my experiments have led me, there is every reason to believe that the electrical action of the battery depends on these changes.

That we may present a distinct view of this curious investigation, it will, perhaps, be advisable to consider the phenomena of the pile in the order they may be supposed to follow. This may be done by a few remarks on *the transition of zinc from the state of a metal to that of an oxide; the transition of the oxygen from the state in which it exists in water to that in the oxide of zinc; and the change effected by the union of the acid in the solution with the oxide of zinc.* To pursue this subject further, it might be advisable to trace the influence of these changes of form on the remaining phenomena of the pile, such as the *influence of the formation of the oxide upon the zinc; the influence of the zinc upon the copper, the copper upon the hydrogen,* and lastly, the *action of these upon the decomposing power of the pile.*

On the transition of the zinc from the metallic state into that of an oxide. This effect, there is every reason to believe, is the primary cause of voltaic action in the battery. The most remarkable circumstance attending this change of state is an increase of volume; the density of the oxide of zinc being about 3 and that of the metal about 7. Now the space occupied by any body must always be inversely as the density; the increase of volume, therefore, which results from this transition is as 7 to 3.

It is generally supposed that all substances possess electricity in a certain state as to intensity and quantity, and have a certain ratio to the fluid in surrounding bodies, when in a non-electric state. When any substance receives an additional quantity, or has a part of its natural electricity abstracted, it is said to be electrified; in the former case positively, in the latter negatively.

From this opinion, it would necessarily follow, that when the zinc passing from the metallic state to that of an oxide, obtains a volume represented by 7 instead of 3, the quantity of its electric fluid, in order to retain the non-electric state, or in other words that of equilibrium, ought to be as 7, but it remains only as 3. The zinc therefore having a less amount of electricity in proportion to its bulk must be negatively electrified.

On the transition of the oxygen from the state in which it exists in water to that in the oxide of zinc. This effect is accompanied by an increase of density; the density of water being 1 and that of the oxide of zinc about 3. The spaces occupied by the oxygen before and after the transition are as I have found by experiment in inverse ratios, being in one instance 1 : 3 and in the other 3 : 1.

What influence then has this diminution of bulk on the electrical condition of the oxygen? The ratio which the oxygen when it forms water in combination with hydrogen, bears to surrounding bodies, as respects the quantity of the electric fluid necessary to maintain equilibrium, cannot be maintained when it combines with the zinc, and forms an oxide, the quantity being then as 3 : 1. The oxygen under such circumstances must be positively electrified, having the fluid in excess.

Hence then it will appear that the effect on the oxygen is the reverse of that on the zinc; the former being in the positive, and the latter in a negative state when the oxide of zinc is forming.

On the change effected by the union of the acid in solution with the oxide of zinc. As far as our information extends respecting the action of acids upon the oxides of metals, there can be no doubt that the compound thus formed has a greater density than the sum of the densities of the two constituents—the acid and the oxide. When the union of the oxide of zinc with the acid of the solution takes place, there will be an increase of density and consequently a diminution of volume. The resulting compound containing an excess of fluid above equilibrium will be positively electrified.

On the electrical state of the metallic zinc in relation to the oxide of zinc formed from the same plate. The oxide of zinc immediately on its formation must contain, owing to the expansion of the metal itself, a less quantity of electricity than it should do in relation to surrounding bodies. It must therefore be in a negatively electrified state. The metallic zinc, remaining after the formation of the oxide, must be in a positive state, owing to what in the ordinary electrical language is called induction.

On the electrical state of the copper in relation to the zinc. The metallic zinc being positively electrified to the oxide of zinc, it must be in a negative state to all surrounding bodies, and among others to the copper with which it is in contact agreeably to the principles of induction.

*On the transition of the hydrogen from the condition in which it exists in water to its uncombined gaseous state.** The hydrogen in the state of gas occupies a space which may be represented as 12,000; and in the state of water as 1. Hence it must follow that the hydrogen gas must contain less fluid than surrounding bodies, and is therefore electrified negatively.

* One cubic foot of water weighs 1000 ounces, which multiplied by 438 yields the number of grains 438,000. One cubic foot of hydrogen gas weighs about 38 grains. 438,000 divided by 38 yields 11,526, which is the ratio of the density of water to hydrogen gas. This therefore is the amount of the expansion which hydrogen undergoes during its transition from the state of water to that of gas.

It now becomes a question how the hydrogen can be brought to that positive state in which we know it to exist, as it is always attracted to the negative pole of the battery. It may be said that it is owing to the hydrogen being under the inductive power of the negative copper; but we have no evident reason why the copper should exercise that inductive power upon the hydrogen and not upon the other components of the arrangement. Induction may help us to a statement of the fact but gives us no assistance in an attempt to discover the cause.

From what has been already stated, it will be evident that metallic zinc, in its transition into the state of an oxide, undergoes expansion; that when oxygen is separated from its combination with hydrogen forming water and combines with the zinc, contraction will be the result; and the same result is produced upon the zinc and copper as well as when the oxide and acid are resolved into a saline compound. That there is some connexion between these changes and the electrical properties of the battery is undeniable; but I am not aware that any attempt has ever been made to trace out the connexion. It is true that an explanation is generally given by the assistance of the doctrine of induction, which supposes an electrified body to induce an opposite state upon the substances brought near it. Thus the poles of the battery may be considered as in opposite states, and all the electric actions in the solution as the result of their inductive influence. But it may be doubted whether the frequent use which is now made of the principles of induction may not retard the progress of electrical science. The word *induction* may be of use as connecting many isolated facts, but it gives us no insight into the cause of the phenomena attributed to the proximity of bodies to electrified substances. The frequent use of the word is therefore mischievous, as it prevents enquiry and serves only to hide our ignorance.

II. Explanation of these changes on the Theory of Vibration.

1. In order to render this explanation more intelligible, we must suppose every body to be capable of undergoing vibration, consisting of a contracting, and an expanding stage; being positive and imparting the fluid during the former; and negative and receiving it during the latter.

As soon as the acid acts upon the zinc, the oxide formed will have its tendency to undergo the expanding stage increased, as already shown; thereby becoming highly negative. The fluid will be powerfully abstracted from the remainder of the metal zinc, which has therefore its tendency to undergo the contracting stage increased. While these two stages in each exist, a current as at *a*, fig. 1, Plate I. will pass between them. When this current ceases, the opposite, the expanding stage commences in the zinc, it becomes negative, and absorbs the fluid from the copper which in turn becomes positive, thus current *b* is generated. When this ceases, the opposite, the expanding stage commences, it becomes negative, absorbing the fluid from the solution S which thereby becomes positive, generating the current *c*.

The general effect of this vibration will be, that a current will exist through the arrangement, from the zinc, through the solution, through the copper to the zinc again, whereby a complete circuit will be established.

It has been already mentioned, that the hydrogen at the moment of its transition from the state of water to that of gas, undergoing an expansion equal to 12,000, must be highly negative; which appears contrary to fact, as it is highly positive being attracted by the negative copper. The theory of vibration enables us to meet this difficulty. This gas being at the moment of its formation the

most elastic body present, will absorb the fluid from the others, more particularly the solution, and will also, owing to its great elasticity, more readily subsequently undergo compression, become positive and give off its fluid to the negative copper.

If the above theory of vibration be true, it follows, that as the consumption of the fluid by the oxidizement of the zinc is not met by an equivalent production of the fluid within the battery, the action of the battery must depend upon its power of absorbing the fluid from surrounding bodies, rather than imparting it to them. This is highly probable from a most important experiment of Dr. Faraday, related in section 728 of his Experimental Researches. "If the acid be strong then a remarkable disappearance of oxygen took place; thus one made by mixing two measures of strong oil of vitriol with one of water, gave 42 volumes of hydrogen, but only 12 of oxygen." Now the inference which Dr. Faraday draws from this experiment that the deutoxide of hydrogen, or oxywater of Thenard, is formed, is highly probable. It may be proper to state that this compound consists of 1 hydrogen and 16 oxygen; water consists of the same, but the proportions different, 1 hydrogen and 8 oxygen. The former containing double the quantity of oxygen that the latter does. When water is decomposed, it yields 2 volumes of hydrogen and 1 of oxygen. In the above experiment 42 volumes of hydrogen are produced, but only 12 of oxygen instead of 21. What has become of the 9 volumes of oxygen? The only probable inference is, that it exists in combination with the water of the solution as the deutoxide. This compound can exist *only* at low temperatures, for as the temperature rises, oxygen is given off, and water alone remains. The presumption therefore is, that this compound which can exist at low temperatures *only*, is formed by the action of the battery, which is dependant upon its power of absorbing the fluid from surrounding bodies, rather than of imparting it to them. That this view of this action is the true one, the magnet furnishes further evidence. The lower the temperature, the stronger the power of the magnet, as Dr. Faraday has proved by an experiment related in the article Magnetism, section 51, published by the Society for the Diffusion of Useful Knowledge; and accordingly we find, that the wire of the battery, as when forming the common helix, converts a plain piece of steel as a common needle into a magnet; not by imparting the fluid, but by absorbing it. These two facts, these two analogous instances, appear to place the foregoing statement upon an incontrovertible basis, viz., that the battery forms oxywater, and also the magnet, by its power of absorption of the fluid.

I am aware that an objection has been made to the theory of vibration, because it cannot be proved by *experiment* to exist, and there is certainly not any experiment sufficiently delicate to determine its existence; but the great velocity with which these vibrations occur, preclude the possibility of direct observation; as thousands or even millions of them may occur within a second of time. The velocity of electricity has been estimated differently by different philosophers; but the determinations of Professor Wheatstone appear most worthy of dependance; viz. that the velocity of electricity through a copper wire exceeds that of light through planetary space.

It may now be stated that if the theory of vibration is true, it will follow, that as the positions and times of the two stages do not correspond, the *fluid* given out, and absorbed, during the contracting and expanding stages respectively, *will not be equal.*

Now, this, as respects sound, which is almost universally acknowledged to be dependant upon the vibration of the matter through which it passes, is known to be the fact. La Place, while investigating mathematically, the phenomena of sound, found that his *results* did *not correspond* with the *facts.* In Mrs. Somerville's Work, page 155, are these observations. "In dry air at the freezing tempera-

ture, sound travels at the rate of 1089 feet in a second, and at 62° of Fahrenheit its speed is 1123 feet in the same time. It was found, however, that the velocity of sound determined by observation, exceeded what it ought to have been theoretically by 173 feet or about one sixth of the whole amount. La Place suggested that this discrepancy might arise from the increased elasticity of the air, in consequence of a development of latent heat, during the undulations of sound; and the result of calculation fully confirmed the accuracy of his views."

Were the name of La Place only known to us from the fact of his having made this observation, it would deserve immortality; for it explains to us, that the heat given out during the compression of the air, is not absorbed during the rarefaction necessarily accompanying it: for it is to be recollected, that the volume of the air remains not permanently contracted or compressed, but the same after the sound has passed through it, as before.

We are thus informed, not in the imperfect dialect of man, but in the universal language of nature, that this heat generated during the transmission of sound is a test of the presence of vibration.

Fig. 2, Plate I. A B, an arrow denoting the direction of the force generating the vibration. C, the positions of the contracting stages of vibration of the matter through which the force acts. E, The positions of the expanding stage of vibration. Each vibration supposed to be constituted of the two stages, as from *a* to *b*. The arrows denote the fluid flowing in or out of the respective positions where the stages occur. The numbers denote the periods of time when the separate stages are supposed to occur.

From the diagram, it is evident, that as the positions and times of the stages differ, they cannot interfere: thus for instance, the contracting stage at 3 occurs too late to interfere with the expanding stage at 2, and too early to interfere with that at 4, because this last stage is not as yet supposed to be in existence. To this inevitable conclusion we are driven; that the expanding, cannot interfere with the contracting stages, because time and place do not permit it. That the expanding stage will require the fluid is undoubtedly true; but it is also as true, that the same fluid which is given out during the contracting stage is not all absorbed during the expanding stage. Hence the latent heat observed by La Place.

But here an objection may be started; that although the above observations may be justified from that of La Place, as respects sound, still we are not justified in extending the same by analogy, to the explanation of the phenomena of electricity. I believe we are fully justified. Sound, electricity, magnetism, light, and heat, are each and all in connexion with a force or motion through matter, and consequently also with its vibration. We have therefore yet to learn the existence of electrical phenomena independent of the presence of matter.

Heat therefore becomes a test of the vibration in matter. Thus the heat generated by a voltaic battery, denotes the vibration taking place in it; in the same manner as the heat observed by La Place was a test of the vibration accompanying the motion through air connected with sound; being that given out during the compression, but not absorbed during the rarefaction of the air, otherwise that heat could not have been observed.

From the foregoing investigation, two inferences are to be drawn, whose mutual influence upon the action of the voltaic battery is most important, and in the present imperfect state of electrical knowledge cannot be kept too constantly in view.

1*st. Inference.* That by the vibration existing in the battery, fluid is disengaged, whereby it manifests heating power in its action upon bodies.

2d. Inference. That the battery, owing to the consumption of the fluid in it, by the oxidizement of the zinc, absorbs the fluid from bodies exposed to its action.

These two inferences lead to an inconsistent result. The first inference implies that the battery imparts fluid to bodies exposed to its influence. The second, that it receives the fluid from them.

We accordingly find that batteries relatively to these two inferences, and their respective tendencies, may very conveniently be divided into two classes. One, including those whose action upon bodies is mainly dependant upon the heat they communicate to them. The other, those whose action is mainly electric and chemical. The first class includes *quantity* ; the second *intensity* batteries.

As a *quantity* battery, Hare's calorimotor stands first. In it the influence of vibration predominates. It heats and deflagrates the metals and oxidizes them. It possesses no electric, no chemical action, giving no shocks, producing no decomposition. All this occurs, because, owing to the intensity of its vibration, its power of imparting the fluid to surrounding bodies overpowers that of abstracting it from them.

In an *intensity* battery, the power to absorb the fluid from bodies exceeds that of imparting it to them. It produces electric and chemical action. It gives shocks, it decomposes bodies, while its influence dependant upon vibration, is much inferior to that of a quantity battery.

We now see the reason why a small battery should produce such extraordinary effects upon electro-magnets, which are scarcely exceeded by the action of a larger battery. It is because the interference arising from the heat disengaged during the vibration is proportionably less in a small battery than in a large one; the electric action is therefore proportionably greater.

We now see the reason why the sustaining battery is so well adapted for electro-magnetic and decomposing purposes. It is because so little interference arises from the fluid given off during vibration, by which its electric and decomposing action might be impeded.

We now see the reason why batteries composed of metals and water alone, should (as when employed by Mr. Crosse), produce crystals, thereby rivalling nature herself. It is because in such batteries the interference by the action of heat liberated by vibration is so slight, the electric action goes on undisturbed. No fact is scarcely better known, than that the abstraction of heat favours crystallization.

We also see the reason why the action of batteries made with sulphuric or nitric acids, must of necessity be short-lived. It is because by the intensity of their vibration and the accompanying production of heat, they counteract themselves.

III. The distinction between the Terms, Quantity, and Intensity, determined by the Theory of Vibration.

When the fluid given off, during the contracting stage is not counteracted, by being taken up, during the expanding stage, we get those effects which we term those of quantity; when it is counteracted, we get those of intensity.

Batteries of single pairs of plates are most proper for quantity. Those with numbers or series of pairs, for intensity. The cause of this will appear.

The calorimotor, the best sample of a quantity battery, consists of two plates, copper and zinc rolled up together, and acid. Now if we consider each separate plate, to be undergoing vibration, contracting on one side or end, and expanding on the opposite, it must be very evident that very little interference owing to the distance, can occur between them, and that this interference will be less

as the size of the plate is greater. But in a battery composed of a series, interference will occur between the different stages, and to a greater extent and in a greater ratio, as the number of series is greater. This subject may be illustrated by a diagram.

Fig. 3, Plate I. Q, intended to represent distance, as that of the surface of a plate belonging to a quantity battery. I, a series of plates, belonging to an intensity battery. C, positions undergoing the contracting stages of vibration. E, positions undergoing the expanding stages of vibration. Z and N, the positive and negative ends of the plates.

The arrows show the direction of the current supposed to pass along the plate or series. The numbers express the time when the vibration in each plate is supposed to occur.

Thus the fluid given out at C′, cannot be much affected by any action occurring at E′, owing to the distance; and any body near C′ may receive some of that fluid which C′ has in excess without any reference to the deficiency at E′. But in the series it is different. If, for instance, the fluid be given out at C. 5, although as in the former instance, it may occur without much reference to the expanding stage at E. 5, still it is more likely to pass to E. 6, where the fluid is deficient than to any body near it, as it would in the former instance. This teaches us why, from the action of a quantity battery, heat ought to result; and why it is less likely in an intensity battery. To this conclusion we come, that in a single pair, heating effects are in a greater ratio to the electrical and chemical; and in a series, in a less ratio; owing to the power of a single pair to impart fluid to bodies, agreeably to the first inference we draw; and that of a series to abstract it from them, agreeably to the second inference.

Much confusion has arisen from inattention to this twofold action of the voltaic battery, and much surprise has been manifested that quantity batteries should show such enormous powers when acting upon good conductors, and at the same time, such weak electrical powers; when it was supposed that the only difference between a quantity, and an intensity battery was, that in the former instance the electric fluid was diffused over a larger surface, and in the latter more concentrated.

This we now see is not the fact. The phenomena of the battery when imparting the fluid to bodies, are analogous if not identical with those of heat. The term quantity as applied to the electrical action of the battery is altogether improper, for instead of expressing a similar force differing in degree merely, it is an antagonist force, exactly in the same manner as we are compelled to consider the force of heat, antagonist to that of crystallization. Hence has arisen the uncertainty and confusion by which the progress of electrical science has been retarded. Hence the opposing theories, which by their conflict have shown that neither were right, because both had equally confounded antagonist, with forces differing merely in degree. The history of electrical controversy is almost entirely constituted of the details of this lamentable mistake.

The following observations in Donovan's Galvanism, page 251, are highly instructive. In objecting to the hypothesis of Volta the following experiment is related. "With a battery of forty pairs of plates each 18 inches square, the effect on the gold leaf electrometer was barely sensible, yet two pairs of plates will ignite and fuse some inches of platina wire." It is afterwards observed "but if the wire be fused by the intensity of the electricity, or what is the same by the condensation of a large quantity upon a small surface, how are we to account for the non-effect on the electrometer, for the leaves separate with the feeblest intensities?"

The above is a sample of the evils arising from the confounding the heating with the electrical power of the battery. We now know the reason why two pairs of plates should ignite some inches

of platina wire, while forty plates should barely influence the electrometer. It is because these two actions depend upon antagonist forces.

IV. Various objections made to the foregoing theory.

Objection 1. *Want of experiments to show the changes of form in the battery referred to, to take place.* It would be difficult to show the changes as they are assumed to occur in the battery, but we have no reason to suppose that the solution of zinc formed by the action of the battery differs from the same product formed in any other way, as by forming the oxide from the metal zinc and dissolving this oxide in the acid employed: the result being the same in either case. That the metal zinc undergoes expansion in forming the oxide, can be very readily shown by taking the specific gravities of the metal and oxide, and by adding a given weight of each to water in a common phial and observing the difference of elevation by a graduated scale.

Objection 2. *Chemical Action not the cause of all Electrical Action.* Chemical action appears to be the cause of the electric action of the battery, for we find, that if the former be diminished, the latter is reduced. But we are not to suppose that chemical action is the sole cause of electric effect. The present state of our electrical knowledge is far from justifying us in drawing such an inference. The cause of electric action may be more deeply seated, as it appears to depend upon the vibration of matter, which by disturbing the equilibrium of the quantity of fluid, produces electric effect. If, therefore, we are to found electric action upon the basis of chemical action, we must suppose that the arrangement of the atoms of matter during its vibration undergoes a chemical change. This probably may turn out to be the truth, but with our present knowledge is incapable of demonstration.

Objection 3. *That Electric Phenomena may be explained by the examination of the Properties of Matter, independently of any fluid.* Although it might be possible to explain the phenomena of the battery, agreeably to the commonly received laws of matter, yet it appears much more consistent with the fertility and variety which nature employs in her operations, that such a species of matter called an electric fluid should exist. The balance of probability seems strongly in its favour. If we suppose a matter whose parts are so intensely small, that the attractive force between themselves and surrounding matter exceeds that within themselves, we appear to possess all that we require to explain the phenomena dependant upon an electric fluid. Such matter must be highly elastic, highly diffusive, strongly disposed to pass in the direction of least resistance.

Objection 4. *That if two Plates of Zinc, one simple, and the other amalgamated, be placed in the same acid solution, and connected with the galvanometer, the current generated would be in the direction different from what it ought according to the preceding views.* In the foregoing paper I have confined my observations to the voltaic battery as commonly arranged. In this experiment, I see nothing subversive of the theory of vibration, but I think confirmatory of it, if the thermo-electric condition of the arrangement be examined. We will shortly reconsider this objection.

No theory hitherto advanced has explained the many anomalies attending the action of the voltaic battery upon the wire and needle when forming the galvanometer. Thus the deflections of the needle during the same experiment frequently occur in opposite directions. The theory of vibration enables us to meet these anomalies, and to show that they constitute its necessary consequence.

In Mr. Sturgeon's Experimental Researches are many of these anomalies. Thus the deflections of the needle when the acid employed is weak is frequently the reverse of that occurring when strong. The experiments in paragraphs 11 and 12 strongly illustrate this. A small wooden box B, fig. 4;

Plate I, divided in the middle by a diaphragm *d*, thus forming two chambers; in the one A is acid; in the other W is water. In each is immersed a plate of copper *a* and *b*, each connected by the wires *w w*, with the galvanometer *g*, of which *n* is the needle. In experiment 11, 40 or 50 parts of water to one of nitrous acid are employed; in experiment 12, equal parts. In experiment 11, the deflection of the needle denotes that the current *o o*, passes from the plate *b*, in the chamber W, to plate *a*, in the chamber A. In experiment 12, the deflection denotes that the current *v v* is passing in the opposite direction. Mr. Sturgeon subsequently observes that when strong acid is employed, the deflection at the commencement is the same as with weak acid, but after some time undergoes this vicissitude.

The theory of vibration teaches us the cause of this difference in the direction of the two currents, and the consequent difference in the deflection of the needle. If, as in experiment 11, the acid employed be weak, the oxidizement of the plate *a*, will proceed slowly, the resulting vibration will be weak, the quantity of fluid liberated will be small, and will consequently offer little interruption to the current *o o*, generated by the above oxidizement. But when on the contrary, as in experiment 12, the acid in A is strong, the resulting vibration will be strong, the quantity of fluid liberated will be large, the current *o o*, will be overpowered, and the current *v v*, flowing in the opposite direction, will be established. We also see, why, as Mr. Sturgeon has observed, time is essential even with the strong acid for the development of the contrary current *v v*. It is because the latter is the creature or effect of the former current.

If we substitute two plates of zinc, one plain *a*, and the other amalgamated *b*, for those of copper, and remove the diaphragm, employing the acid solution alone, the deflection of the needle is such as denotes the existence of the current *v v*, not *o o*, although the negative plate *a* becomes oxidized, the positive plate *b* being scarcely acted upon. This is in direct opposition to the chemical theory of electricity. This experiment appears to be in accordance with the theory of vibration, and its generated current, *v v*. The amalgam of zinc does not, as might at first be supposed, occupy the place of the copper, the negative metal in the standard battery; for we are informed by Jacobi (Annals of Electricity, pages 429 and 431), that the amalgams are positive to their constituent metals. According to this view, the current ought to pass in the direction *v v*, as it is found to do in this experiment.

OBJECTION 5. *That when water instead of acid was employed in the calorimotor, by its action, crystals had been produced by Mr. Crosse.* In this experiment, owing to the weak vibration, the heat disengaged will be so small as scarcely to interfere with the action of the battery abstracting the fluid from bodies, owing to the oxidizement of the zinc. Hence the crystals.

Note.—Since the commencement of the foregoing paper, I have met with some views of Mr. Karsten, of Berlin, of the effects of electricity by contact, which are highly in accordance with those advanced. That metals and probably all solid bodies become positively electrified when immersed in fluids. That a solid partially immersed in a fluid, acquires electric polarity; the part not immersed being negative, and the other positive. That solid bodies differ greatly in their electromotive power in regard to the same fluid; and this difference is the true cause of the electric, chemical, and magnetic action in the galvanic circuit.

II. *Description of some Experiments made with the Voltaic Battery, by* ANDREW CROSSE, ESQ *of Broomfield, near Taunton, for the purpose of producing Crystals; in the process of which Experiments certain Insects constantly appeared. Communicated in a Letter dated 27th December,* 1837, *addressed to the Secretary of the London Electrical Society.*

Read 20th January, 1838.

My dear Sir,

I trust that the gentlemen who compose the "Electrical Society" will not imagine that because I have so long delayed answering their request, to furnish the Society through you, as its organ, with a full account of my electrical experiments, in which a certain insect made its unexpected appearance, that such delay has been occasioned by any desire of withholding what I have to state from the Society in particular, or the public at large. I am delighted to find that at last, late, though not the less called for, a body of scientific gentlemen have linked themselves together for the sake of exploring and making public those mysteries, which hitherto, under a variety of names, and ascribed to all causes but the true one, have eluded the grasp of men of research, and served to perplex, perhaps, rather than to afford sufficient data to theorise upon. It is true that much has been done in the course of a few years, and that which has been done only affords the strongest reason for believing that vastly more remains to be done. It would be presumptuous in me to enumerate the services of a Davy, a Faraday, and many other great men at home, or a Volta and an Ampère, with a host of others abroad. These distinguished men have laid the foundations, on which their successors ought to endeavour to erect a building worthy of the scale in which it has been commenced. Electricity is no longer the paltry confined science which it was once fancied to be, making its appearance only from the friction of glass or wax, employed in childish purposes, serving as a trick for the school boy, or a nostrum for the quack. But it is, even now, though in its infancy, proved to be most intimately connected with all operations in chemistry, with magnetism, with light and caloric; apparently a property belonging to all matter, perhaps ranging through all space, from sun to sun, from planet to planet, and not improbably the secondary cause of every change in the animal, mineral, vegetable, and gaseous systems. It is to determine whether this be or not, the case, as far as human faculties can determine, to ascertain what rank in the tree of science electricity is to hold; to endeavour to find out to what useful purposes it might be applied, that I conceive is the object of your Society, and I shall at all times be ready and willing, as a member, to contribute my quota of information to its support, knowing well, that however little it might be, it will be as kindly received as it is humbly offered. It is most unpleasing to my feelings to glance at myself as an individual, but I have met with so much virulence and abuse, so much calumny and misrepresentation, in consequence of the experiments which I am about to detail, and which it seems in this *nineteenth century* a crime to have made, that I must state, not for the sake of myself (for I utterly scorn all such misrepresentations), but for the sake of truth and the science which I follow, that I am neither an "Atheist," nor a "Materialist," nor a "self imagined creator," but a humble and lowly reverencer of that Great Being, whose laws my accusers seem wholly to have lost sight of. More than this, it is my conviction, that science is only valuable as a mean to a greater end. I can assure you, sir, that I attach no particular value to any experiment that I have made, and that my feelings and habits are much more of a retiring than an obtruding character; and I care not if what I have done be entirely overthrown, if truth be elicited. The following is a plain and correct account of the experiments alluded to.

In the course of my endeavours to form artificial minerals by a long-continued electric action on fluids holding in solution such substances as were necessary to my purpose, I had recourse to every

variety of contrivance which I could think of, so that, on the one hand, I might be enabled to keep up a never-failing electrical current of greater or less intensity, or quantity, or both, as the case seemed to require; and on the other hand, that the solutions made use of should be exposed to the electric action in the manner best calculated to effect the object in view. Amongst other contrivances, I constructed a wooden frame, of about two feet in height, consisting of four legs proceeding from a shelf at the bottom supporting another at the top, and containing a third in the middle. Each of these shelves was about seven inches square. The upper one was pierced with an aperture, in which was fixed a funnel of Wedgewood ware, within which rested a quart basin on a circular piece of mahogany placed within the funnel. When this basin was filled with a fluid, a strip of flannel wetted with the same, was suspended over the edge of the basin and inside the funnel which, acting as a syphon, conveyed the fluid out of the basin, through the funnel, in successive drops. The middle shelf of the frame was likewise pierced with an aperture, in which was fixed a smaller funnel of glass, which supported a piece of somewhat porous red oxide of iron from Vesuvius, immediately under the dropping of the upper funnel. This stone was kept constantly electrified by means of two platina wires on either side of it, connected with the poles of a Voltaic battery of nineteen pairs of five-inch zinc and copper single plates, in two porcelain troughs, the cells of which were filled at first with water and $\frac{1}{500}$ of hydrochloric acid, but afterwards with water alone. I may here state, that in all my subsequent experiments relative to these insects, I filled the cells of the batteries employed with nothing but common water. The lower shelf merely supported a wide-mouthed bottle, to receive the drops as they fell from the second funnel. When the basin was nearly emptied, the fluid was poured back again from the bottle below into the basin above, without disturbing the position of the stone. It was by mere chance that I selected this volcanic substance, choosing it from its partial porosity; nor do I believe that it had the slightest effect in the production of the insects to be described. The fluid with which I filled the basin was made as follows.

I reduced a piece of black flint to powder, having first exposed it to a red heat and quenched it in water to make it friable. Of this powder I took two ounces, and mixed them intensely with six ounces of carbonate of potassa, exposed them to a strong heat for fifteen minutes in a black lead crucible in an air furnace, and then poured the fused compound on an iron plate, reduced it to powder while still warm, poured boiling water on it, and kept it boiling for some minutes in a sand bath. The greater part of the soluble glass thus fused, was taken up by the water, together with a portion of alumina from the crucible. I should have used one of silver, but had none sufficiently large. To a portion of the silicate of potassa thus fused, I added some boiling water to dilute it, and then slowly added hydrochloric acid to supersaturation. A strange remark was made on this part of the experiment at the meeting of the British Association at Liverpool, it being then gravely stated, that it was impossible to add an acid to a silicate of potassa without precipitating the silica! This, of course, must be the case, unless the solution be diluted with water. My object in subjecting this fluid to a long-continued electric action through the intervention of a porous stone, was to form, if possible, crystals of silica at one of the poles of the battery, but I failed in accomplishing this by those means. On the fourteenth* day from the commencement of the experiment, I observed, through a lens, a few small whitish excrescences or nipples projecting from about the middle of the electrified stone, and nearly under the dropping of the fluid above. On the eighteenth* day these projections enlarged, and seven or eight filaments, each of them longer than the excrescence from which it grew, made their appearance on each of the nipples. On the twenty-second* day these appearances were more elevated and

* Plate I. denoted by the fig. 14 18, 22, and 26.

distinct, and on the twenty-sixth* day each figure assumed the form of a perfect insect, standing erect on a few bristles which formed its tail. Till this period I had no notion that these appearances were any other than an incipient mineral formation; but it was not until the twenty-eighth day, when I plainly perceived these little creatures move their legs that I felt any surprise, and I must own that when this took place, I was not a little astonished. I endeavoured to detach with the point of a needle, one or two of them from its position on the stone, but they immediately died, and I was obliged to wait patiently for a few days longer, when they separated themselves from the stone, and moved about at pleasure, although they had been for some time after their birth apparently averse to motion. In the course of a few weeks about a hundred of them made their appearance on the stone. I observed that at first each of them fixed itself for a considerable time in one spot, appearing, as far as I could judge, to feed by suction; but when a ray of light from the sun was directed upon it, it seemed disturbed, and removed itself to the shaded part of the stone. Out of about a hundred insects, not above five or six were born on the south side of the stone. I examined some of them with the microscope, and observed that the smaller ones appeared to have only six legs, but the larger ones eight. It would be superfluous to attempt a description of these little mites when so excellent a one has been transmitted from Paris. It seems that they are of the genus *Acarus*, but of a species not hitherto observed. I have had three separate formations of similar insects at different times, from fresh portions of the same fluid, with the same apparatus. As I considered the result of this experiment rather extraordinary, I made some of my friends acquainted with it, amongst whom were some highly scientific gentlemen, and they plainly perceived the insect in various states. I likewise transmitted some of them to one of our most distinguished physiologists in London, and the opinion of this gentleman, as well as of other eminent persons to whom he showed them, coincided with that of the gentlemen of the Academie des Sciences, as to their genus and species. *I have never ventured an opinion as to the cause of their birth*, and for a very good reason—I was unable to form one. The most simple solution of the problem which occurred to me, was, that they arose from ova deposited by insects floating in the atmosphere, and that they might possibly be hatched by the electric action. Still I could not imagine that an ovum could shoot out filaments, and that those filaments would become bristles; and moreover, I could not detect, on the closest examination, any remains of a shell. Again, we have no right to assume that electric action is necessary to vitality, until such fact shall have been most distinctly proved. I next imagined, as others have done, that they might have originated from the water, and consequently made a close examination of several hundred vessels, filled with the same water as that which held in solution the silicate of potassa, in the same room, which vessels constituted the cells of a large Voltaic battery, used without acid. In none of these vessels could I perceive the trace of an insect of that description. I likewise closely examined the crevices and most dusty parts of the room with no better success. In the course of some months, indeed, these insects so increased, that when they were strong enough to leave their moistened birth-place, they issued out in different directions, I suppose, in quest of food; but they generally huddled together under a card or piece of paper in their neighbourhood, as if to avoid light and disturbance. In the course of my experiments upon other matters, I filled a glass basin with a concentrated solution of silicate of potassa without acid, in the middle of which I placed a piece of brick, used in this neighbourhood for domestic purposes, and consisting mostly of silica. Two wires of platina connected either end of the brick with the poles of a Voltaic battery of sixty-three pairs of plates, each about two inches square. After many months' action, silica in a gelatinous state formed in some quantity

* Plate I. denoted by the fig. 14, 18, 22, and 26.

round the bottom of the brick, and as the solution evaporated, I replaced it by fresh additions, so that the outside of the glass basin being constantly wet by repeated overflowings, was, of course, constantly electrified. On this outside, as well as on the edge of the fluid within, I one day perceived the well-known whitish excrescence with its projecting filaments. In the course of time they increased in number, and as they successively burst into life, the whole table on which the apparatus stood, at last was covered with similar insects, which hid themselves wherever they could find a shelter. Some of them were of different sizes, there being a considerable difference in this respect between the larger and smaller; and they were plainly perceptible to the naked eye as they nimbly crawled from one spot to another. I closely examined the table with a lens, but could perceive no such excrescence as that which marks their incipient state, on any part of it. While these effects were taking place in my electrical room, similar formations were making their appearance in another room, distant from the former. I had here placed on a table, three Voltaic batteries unconnected with each other. The first consisted of twenty pairs of two-inch plates, between the poles of which I placed a glass cylinder filled with a concentrated solution of silicate of potassa, in which was suspended a piece of clay slate by two platina wires connected with either pole of the battery. A piece of paper was placed on the top of the cylinder to keep out the dust. After many months' action, gelatinous silica in various forms was electrically attracted to the slate, which it coated in rather a singular manner, unnecessary here to describe. In the course of time I observed similar insects in their incipient state forming around the edge of the fluid within the jar, which, when perfect, crawled about the inner surface of the paper with great activity. The second battery consisted of twenty pairs of cylinders, each equal to a four-inch plate. Between the poles of this I interposed a series of seven glass cylinders, filled with the following concentrated solutions:—1. Nitrate of copper: 2. Sub-carbonate of potassa: 3. Sulphate of copper: 4. Green sulphate of iron: 5. Sulphate of zinc: 6. Water acidified with a minute portion of hydrochloric acid: 7. Water poured on powdered metallic arsenic, resting on a copper cup, connected with the positive pole of the battery. All these cylinders were electrically united together by arcs of sheet copper, so that the same electric current passed through the whole of them.

After many months' action, and consequent formation of certain crystalline matters, which it is not my object here to notice, I observed similar excrescences with those before described at the edge of the fluid in every one of the cylinders, excepting the two which contained the carbonate of potassa, and the metallic arsenic; and in due time a host of insects made their appearance. It was curious to observe the crystallised nitrate and sulphate of copper, which formed by slow evaporation at the edge of the respective solutions, dotted here and there with these hairy excrescences. At the foot of each of the cylinders I had placed a paper ticket upon the table, and on lifting them up I found a little colony of insects under each, but no appearance whatever of their having been born under their respective papers, or on any part of the table. The third battery consisted of twenty pairs of cylinders, each equal to a three-inch plate. Between the poles of this I interposed likewise a series of six glass cylinders, filled with various solutions, in only one of which I obtained the insect. This contained a concentrated solution of silicate of potassa. A bent iron wire, one-fifth of an inch in diameter, in the form of an inverted syphon, was plunged some inches into this solution, and connected it with the positive pole, whilst a small coil of fine silver wire joined it with the negative.

After some months' electrical action, gelatinous silica enveloped both wires, but in much greater quantity at the positive pole; and in about eight months from the commencement of the experiment, on examining these two wires very minutely, by means of a lens, having removed them from the solution for that purpose, I plainly perceived one of these incipient insects upon the gelatinous silica

on the silver wire, and about half an inch below the surface of the fluid, when replaced in its original position. In the course of time, more insects made their appearance, till, at last, I counted at once three on the negative and twelve on the positive wire. Some of them were formed on the naked part of the wires, that is, on that part which was partially bare of gelatinous silica: but they were mostly imbedded more or less in the silica, with eight or ten filaments projecting from each beyond the silica. It was perfectly impossible to mistake them, after having made oneself master of their different appearances; and an occasional motion in the filaments of those that had been the longest formed was very perceptible, and observed by many of my visitors, without my having previously noticed the fact to them. Most of these productions took place from half to three-quarters of an inch under the surface of the fluid, which, as it evaporated very slowly, I kept to the same level by adding fresh portions. As some of these insects were formed on the inverted part of the syphon-shaped wire, I cannot imagine how they contrived to arrive at the surface, and to extricate themselves from the fluid: yet this they did repeatedly; their old places were vacated, and others were born in new ones. Whether they were in an imperfect state (except just at the commencement of their formation), or in a perfect one, they had all the distinguishing characteristic of bristles projecting from their bodies, which occasioned the French *savans* to remark that they resembled a a microscopic porcupine. I must not omit to state, that the room in which these three batteries were acting was kept almost constantly darkened. It was not my intention to make known these observations until I myself should be better informed about the matter. Chance led to the publication of an erroneous account of them, which I was under the necessity of explaining. It is so difficult to arrive at the truth, that mankind would do better to lend their assistance to explore what may be worth investigating, than to endeavour to crush in its bud that which might otherwise expand into a flower. In giving this account, I have merely stated those circumstances regarding the appearance of insects, which I have noticed during my investigations into the formation of mineral matters; I have never studied physiology, and am not aware under what circumstances the birth of this class of insects is usually developed. In my first experiment I had made use of flannel, wood, and a volcanic stone; in the last, none of these substances were present. I never, for a moment, entertained the idea that the electric fluid had animated the organic remains of insects, or fossil eggs, previously existing in the stone or the silica; and have formed no visionary theory which I would travel out of my way to support. I have since repeated these latter experiments in a third room, in which there are now two batteries at work. One consisting of eleven pairs of cylinders, made of four-inch plates between the poles of which is placed a glass cylinder, filled with silicate of potassa, in which is suspended a piece of slate between two wires of platina, as before, and covered loosely with paper. Here, again, is another crop of insects formed. The other battery consists of twenty pairs of cylinders, the electric current of which is passed through six different solutions in glass cylinders, in three of which only is the insect formed, viz., 1st. in nitrate of copper; 2d. in sulphate of copper, in each of which the insect is only produced at the edge of the fluid, as far as I can make out; and 3d. by the old apparatus of coiled silver and iron wire in silicate of potassa, as before. There are now forming on the bottom of this positively electrified wire similar insects, at the distance of fully two inches below the surface of the fluid. On examining these, I have lately noticed a peculiar quality they possess whilst in an incipient state. After being kept some minutes out of the solution, they contract their filaments, so as, in some cases, wholly, and in others partially, to disappear. I at first thought they were destroyed; but, on examining the same spots, on the next day, they were as perceptible as before. In this respect, they seem not unlike the zoophytes, which adhere to the rocks on the sea-shore and which contract on the approach of a finger. I may likewise remark, that

I have not been able to detect their eyes, even when viewed under a powerful microscope, although I once fancied I perceived them. The extreme heat of summer and cold of winter do not appear favourable to their production, which succeeds best, I think, in spring and autumn. As in the above account I have occasionally made use of the word "formation," I beg that it might be understood that I do not mean *creation*, or any thing approaching to it. I am not aware that I have any thing more to add, except the few remarks I shall conclude with.

1st. I have not observed a formation of the insect, except on a moist and electrified surface, or under an electrified fluid. By this *I do not mean to assert that electricity has any thing to do with their birth*, as I have not made a sufficient number of experiments to prove or disprove it; and besides, I have not taken those necessary precautions which present themselves even to an unscientific view. These precautions are not so easy to observe as may at first sight appear. It is, however, my intention to repeat these experiments, by passing a stream of electricity through cylinders filled with various fluids under a glass receiver inverted over mercury, the greatest possible care being taken to shut out extraneous matter. Should there be those who blame me for not having done this before, to such I answer that, independent of a host of other hindrances, which it is not in my power to set aside, I have been closely pursuing a long train of experiments on the formation of crystalline matters by the electric agency, and now different modifications of the Voltaic battery; in which I am so interested, that none but the ardent can conceive what is not in my power to describe.

2dly. These insects do not appear to have originated from others similar to themselves, as they are formed in all cases with access of moisture, and in some cases two inches below the surface of the fluid in which they are born; and if a full grown and perfect insect be let fall into any fluid, it is infallibly drowned.

3dly. I believe they live for many weeks: occasionally I have found them dead in groups, apparently from want of food.

4thly. It has been frequently suggested to me to repeat these experiments without using the electric agency; but this would be by no means satisfactory, let the event be what it would. It is well known that saline matters are easily crystallized without subjecting them to the electric action; but it by no means follows that, because artificial electricity is not applied, such crystals are formed without the electric influence. I have made so many experiments on electrical crystallization, that I am firmly convinced in my own mind, that electric attraction is the cause of the formation of every crystal, whether artificial electricity be applied or not. I am, however, well aware of the difficulty of getting at the truth in these matters, and of separating cause from effect. It has often occurred to me how it is that such numbers of animalcules are produced in flour and water, in pepper and water? also, the insects which infest fruit trees after a blight? Does not a chemical change take place in the water, and likewise in the sap of the tree *previous* to the appearance of these insects, and is or is not every chemical change produced by electric agency? In making these observations I seek to mislead no one. The book of nature is opened wide to our view by the Almighty power, and we must endeavour, as far as our feeble faculties will permit, to make a good use of it; always remembering, that however the timid may shrink from investigation, the more completely the secrets of nature are laid bare, the more effectually will the power of that Great Being be manifested, who seems to have ordained, that

"Order is Heaven's first law."

I beg to remain, in the mean time, my dear Sir,

Your's, very sincerely,

ANDREW CROSSE.

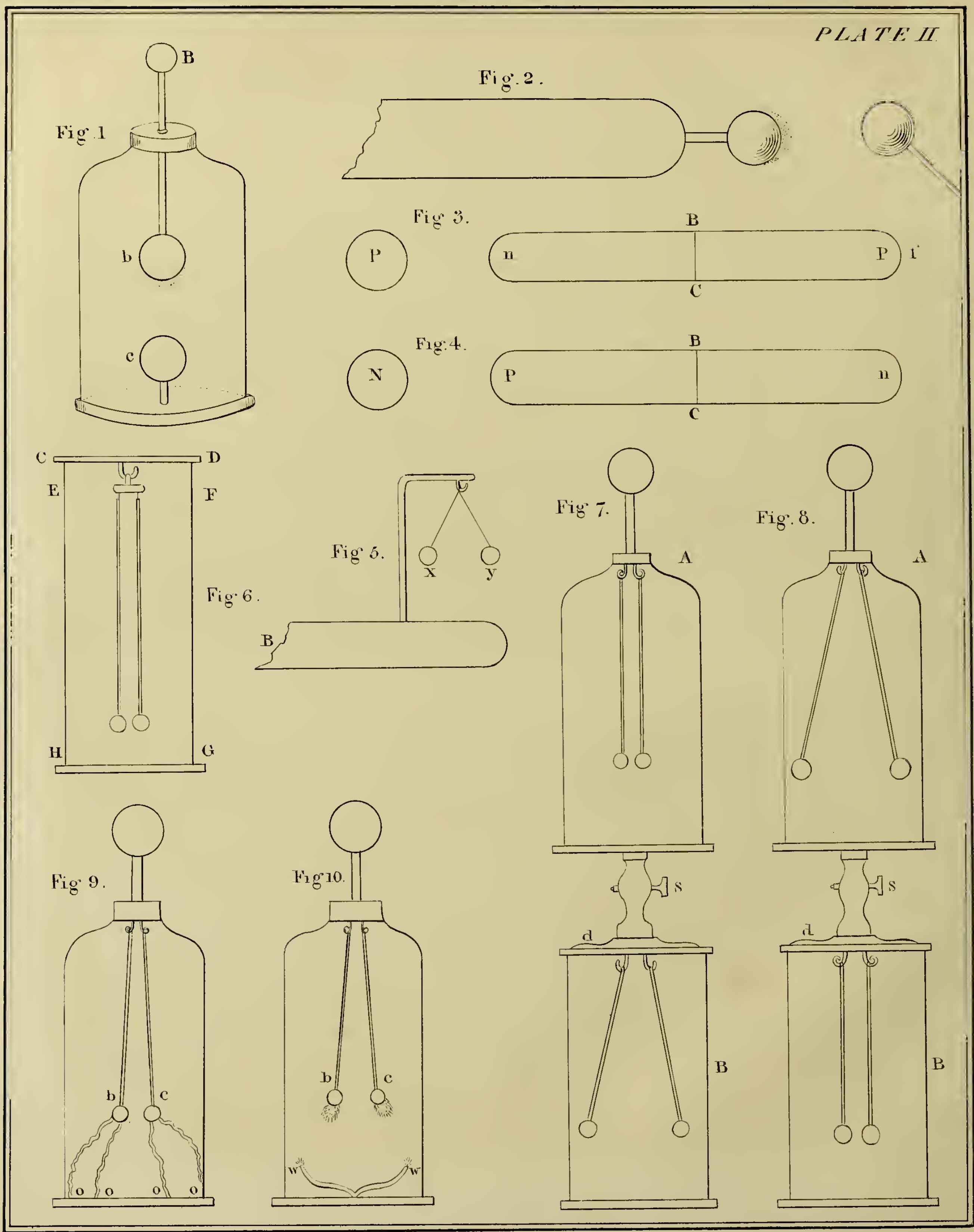
Broomfield,
December 27, 1837.

P.S. Since writing the above account, I have obtained the insects on a bare platina wire plunged into fluo-silicic acid, *one inch below* the surface of the fluid at the negative pole of a small battery of two-inch plates in cells filled with water. This is a somewhat singular fluid for these insects to breed in, who seem to have a flinty taste, although they are by no means confined to silicious fluids. This fluo-silicic acid was procured from London some time since, and consequently made of London water; so that the idea of their being natives of the Broomfield water is quite set aside by this result. The apparatus was arranged as follows; Fig. 7, Plate I. a glass basin (a pint one) partly filled with fluo-silicic acid to the level 1. 2, a small porous pan, made of the same materials as a garden pot, partly filled with the same acid to the level 2, with an earthen cover, 3, placed upon it, to keep out the light, dust, &c. 4, a platina wire connected with the positive pole of the battery, with the other end plunged into the acid in the pan, and twisted round a piece of common quartz; on which quartz, after many months' action, are forming singularly beautiful and perfectly formed crystals of a transparent substance, not yet analyzed, as they are still growing. These crystals are of the modification of the cube, and are of twelve or fourteen sides. The platina wire passes under the cover of the pan. 5, a platina wire connected with the negative pole of the same battery, with the other end dipping into the basin, an inch or two below the fluid; and, as well as the other, twisted round a piece of quartz. By this arrangement it is evident that the electric fluid enters the porous pan by the wire 4, percolates the pan, and passes out by the wire 5. It is now upwards of six or eight months (I cannot at this moment put my hand on the memorandum of the date) since this apparatus has been in action, and though I have occasionally lifted out the wires to examine them by a lens, yet it was not till the other day that I perceived any insect, and there are now three of the same insects, in their incipient state, appearing on the naked platina wire at the bottom of the quartz *in the glass basin at the negative pole.* These insects are very perceptible and may be represented thus (magnified): fig. 8, 1 the platina wire, 2 the quartz, 3 the incipient insects. It should be observed that the glass basin, fig. 7, has always been loosely covered with paper. The incipient appearance of the insect has already been described. The filaments which project are in course of time seen to move, before the perfect insect detaches itself from its birth-place.

(Plate 1, fig. 5,) front view of the filtering apparatus, by the use of which, the insect described made its first appearance. (A. B.) two of the four uprights or legs issuing from the base (C), supporting a moveable shelf (D); which shelf is kept in its place by four pins (E) passing through the four uprights, and may be raised or lowered at pleasure. (F) the top shelf which has an aperture cut in it to receive the Wedgewood ware funnel (G). (H) a quart basin standing on an unseen support within the funnel (G), which support is a circular piece of wood with holes cut in it to allow the free passage of the fluid between the basin and funnel. This basin is filled with the fluid required, which is conveyed out of it by the strip of flannel (I), hanging over the outside of the basin, and inside the funnel, and which, consequently, falls in successive drops through the funnel (G) upon the stone (K), which is supported by the glass funnel (L), kept constantly electrified by the two platina wires (M N), resting on the opposite sides of it, and connected with the opposite poles of a voltaic battery. (O) a wide mouthed bottle standing on the base (C), to receive the fluid as it falls from the second funnel (L). From this bottle, when required, it is poured back again into the basin (H) without disturbing the stone (K).

(Fig. 6, A) a glass cylindrical vessel containing about a quarter of a pint filled with a concentrated solution of silicate of potash. (B) a fine silver wire formed into a coil, which is immersed into the fluid in the cylinder the other end being connected with the negative pole of the battery. (C) an iron wire about 1-fifth of an inch in diameter, bent somewhat in the form of an inverted syphon, immersed in the same vessel, and connected with the positive pole of the battery. (D D) insects in their incipient state making their appearance, some on the gelatinous silica, which partially covers the wire, and some on the naked wire itself. These insects appear magnified.

LONDON ELECTRICAL SOCIETY.



W. Annan lith: 12, Gracechurch St.

III. *Experimental and Theoretical Researches in Electricity. First Memoir. By* WILLIAM STURGEON, ESQ.

SECTION I.

Read December 5th, 1837.

Different opinions of philosophers respecting the nature of electric action—The vibratory Hypothesis examined—Its principles not analogous to those of the hypotheses of Sound and Light—Evidences of the existence of an electric matter.

1. The memoir which I am now about to offer to the notice of the Electrical Society may be considered as the first of a series which it is my intention to bring forward as speedily as circumstances will permit. These memoirs will exhibit a selection and arrangement of facts, which, if I have not deceived myself, can hardly fail to have some weight in the reasonings of those philosophers whose opinions are not yet reconciled to each other respecting the *modus operandi* in the production of certain electrical phenomena.

2. In an enquiry of this kind it often happens that, notwithstanding the apparently trivial circumstance to which it is mainly directed, it becomes essentially necessary not only to notice, but to investigate, certain other points with which it is obviously connected, in order to satisfy the mind respecting the bearing and influence which those points have upon each other; as also, on that which is the principal object of pursuit.

3. Philosophers, of the present day, have taken such extremely dissimilar views of the nature of electric action that they have imposed upon us the indispensableness of a minute retrospection of almost all the variety of phenomena that have hitherto been developed; the laws of whose exhibition necessarily contribute to the establishment of those impenetrable and unalterable principles upon which the science must ultimately rest; and have demanded a reinvestigation of even the very arcanum of all electric action.

4. A certain class of these philosophers who have undertaken the explanation of electric phenomena, consider it requisite to combine the operations of *two* distinct kinds of electric matter, which they have called the *vitreous* and the *resinous*; independently of which, they imagine, no electric phenomena can possibly exist. A second class have contented themselves with the management of *one* fluid only; whilst a third class, still more economical than the preceding, have undertaken the solution of every electric problem, hitherto discovered, independently of the operation of any electric matter whatever; by supposing that electric phenomena are the effects of certain rotatory, or vibratory, motions of the particles of the common matter composing those bodies on which they are displayed. Each of these hypotheses is supported by men of the highest respectability, and of acknowledged talent. They are become subjects of much important discussion amongst electricians of every country; and, therefore, a rigid and impartial investigation of the principles on which they are founded is the only mode of proceeding which can satisfy the mind as to the intrinsic value of their respective peculiarities, and to form a proper estimate of their individual claims to attention. The results of such an enquiry, if properly conducted, can hardly fail to be interesting to the Electrical Society; whilst, in the present instance, it may be regarded as an important preliminary step unavoidably touched upon in the path of research I have ventured to pursue.

5. In an undertaking of this magnitude and importance, under circumstances embracing a balance of authority amongst those who have taken these very different views of electric action, much caution and rigorous circumspection ought necessarily to be observed in every stage of the enquiry.

Moreover, the present infantile state of this Society imperatively demands that a copious selection of obvious and unequivocal experimental data be advanced, and that all reasoning therefrom be plain, lucid, and familiar.

6. Respecting the order in which these hypotheses come under consideration, the precedency would have been but of very little consequence had the probability of truth appeared equally favourable amongst them, or that they had differed from each other in some trifling peculiarity of detail only; but as there is such a wide difference in the very basis of these doctrines, and especially between those which admit of the existence of an electric matter and that which precludes it altogether, it appears essential that the mind becomes perfectly satisfied, as early as possible, respecting the nature of the evidence on which these opposite theoretical views have been founded, and by which they are the most likely to find support; in order that the investigation may be facilitated by disposing of those first which appear to have the least probability in their favour. Moreover, as the existence of an electric matter has, for a long series of years, been acknowledged by almost every philosopher who has paid a sufficient degree of attention to the subject to enable him to form an unbiassed opinion, resting on experience alone: and that the vibratory hypothesis appears more like a novel creation of the imagination than as a doctrine founded on observation and fact, and still remains little more than a confusion of discordant surmises, without, even the least pretensions to systematic organization, or the shadow of either law or rule for the guidance of the electrician, there can appear no impropriety in commencing with a brief enquiry into the extent of interpretation of electrical phenomena, which the latter hypothesis is capable of affording: and afterwards examining that which has so long rested on the supposition of the individuity of an electric matter; and which has obtained a code of laws supposed to be sufficiently explanatory, as far as they proceed, of every fact hitherto developed in certain branches of electricity.

7. If the hypothesis, first for consideration, supposes that electric action depends upon, and emanates from, vibratory movements of the particles of those kinds of matter of which the apparatus employed are usually constructed, such as metal, wood, glass, &c., independently of any other agency, there could be no difficulty in showing its entire fallacy: because it is a fact easily ascertained, that no very energetic electric action, if any at all, can possibly be exhibited by any vibratory movements which those bodies are susceptible of receiving by mechanical means; although those vibrations might be infinitely greater than could possibly be produced by any other mode of procedure. Nor can I think it possible that any electrician, of the present day, whatever may be his theoretical views, or however fond he may appear to be of novel modes of explanation, would be found so far deficient in electrical knowledge as to hazard his fame by an attempt to charge a Leyden jar by the mere vibration of a mass of copper, glass, wood, or any other solid body whatever.

8. If, again, it can be supposed that the charge of a Leyden jar or any other piece of glass, consists in certain tremulous motions of one, or more, of the materials of its structure, communicated from the prime conductor of a machine; is it possible to imagine that those motions would continue during the weeks, nay, even months, that jars have been kept in an electrized condition after they had ceased to be in connexion with any electric apparatus? There is no evidence of such continuous quiverings of the glass, nor is there a fact known to induce a belief that any tremblings exist in the instrument even for a few moments after it has been taken from the machine; nor indeed any whatever, only such as are occasioned by the unavoidable shaking attending the working of the apparatus.

9. Can any one persuade himself that the electric action exhibited by two morsels of metal, by simple contact only, is occasioned by tremulous motions communicated to the metallic particles by the pieces just touching one another? Can he, moreover, stretch his imagination so far as to satisfy

himself that the metals constituting a dry electric column will continue to quake for years after the instrument is first made; and that, notwithstanding all the care that is taken to prevent any motion amongst the materials of the pile, the electric phenomena exhibited by it are the mere effects of a tendency to motion which these materials naturally possess, and which keeps them trembling in spite of all the efforts of the workman, and contrary to the laws of inert matter?

10. I am well aware that the favourites of this hypothesis build much of their reasoning upon supposed analogies: and particularly from the doctrines of sound and light; wherein it is considered that all the variety of phenomena constituting accoustics and optics are the effects of undulatory motions of matter, which, in itself, is neither sound nor light. But it must be borne in mind, that sound is not produced without a first cause. The sonorous body must first be agitated before it can be productive of sound; and the surrounding medium must be agitated to the ear in order to convey the proper impressions to the tympanum, otherwise the sensation of sound cannot exist.

11. Light, also, requires a first cause to shake the medium which is supposed to be productive of it. Moreover, the medium itself is supposed to be *peculiar,* and alone appropriate to the exhibition of the phenomena of light. Hence it is obvious, that unless the electro-undulatory theorists admit of the tremulous motions of some peculiar species of matter, they find but little support from analogy, as far as regards the doctrines of sound and light. And an unequivocal concession of this point, would dispossess the hypothesis of the most material peculiarity discoverable in its structure, by admitting of the existence of an electric matter; and whatever appellation might be conferred on it, the very idea of its existence, and of the indispensableness of its presence in the production of phenomena, would be sufficient to assimilate this hypothesis with those it was intended to subvert. Or if there could be any real difference, it would consist in the substitution of *vibrations* for *transmissions.*

12. As it is not my intention to enter into a historical account of the respective hypotheses which have been contrived for the explanation of electrical phenomena, it will be unnecessary to bring forward the names of individuals who have attempted to support that to which they have been most decidedly attached. Moreover, the hypotheses of Du Fay and Franklin are so well known to electricians that it would be quite unnecessary, at the present day, to go through a detail of the principles on which they are respectively founded. And, although we are very differently circumstanced with respect to the vibratory hypothesis, there being no work in which its principles are intelligibly developed, and consequently no source from which much useful information can be collected, it would be idle to discuss phantom theories which no one would claim; and which, in consequence of the vague indeterminate manner in which they have been ventured upon the credulity of philosophers, it would be difficult to trace to any origin by which they could not, without any refined sophistical dexterity, be very easily evaded. Indeed, I am not aware that any philosopher has attempted to lay down a *plan* for an electrical hypothesis in which a peculiar species of matter is entirely dispensed with: although there are some of them, of considerable eminence, who have ventured an *opinion* that the existence of such matter is not essential to the production of electrical phenomena: assuming that some *undefined* motion amongst the integrant particles of those bodies usually employed in the experiments are alone sufficient for the explanation. It would be exceedingly difficult, if not totally impossible, to form a rational idea of any mode by which such motions of the particles of solid matter could possibly exist. Such an idea appears insusceptible of demonstration, and bears not even the slightest token of probability. But admitting even the existence of these supposed motions, under certain circumstances, they would have to be regarded as *effects* rather than *causes:* which, like all

other effects, would be referrible to some pre-existent force, however mysteriously that force might operate, or however its operations might be concealed from mental perception. The friction suffered by the revolving glass, or the stationary cushion of a machine, might possibly be construed into a cause sufficient to produce the supposed atomical motions: but its efficacy in this respect, would require an elaborate strength of the imagination to continue it to electrized jars long detached from the prime conductor, and far removed from all other electric apparatus.

13. Any attempt to trace the polarity of the dry pile, or the electric condition of a still atmosphere, or any other electro-statical phenomena to the supposed restlessness of the particles of the metals, or of the air, would have to be ventured under the most unfavourable circumstances that could possibly have accompanied it: viz. in the absence of both fact and analogy. Every vertical column of a dry cloudless atmosphere, whatever may be its dimensions, is constantly electro-polar in one and the same direction, having its positive pole upwards.* But it would be difficult to reconcile the mind to a belief that this circumstance is solely owing to the intestine motions amongst the particles of the air, and independently of any other agent. Again, the two sides of a single piece of thin metal, the one bright and the other dull; † or both bright but of different degrees of polish, are as decidedly electro-polar as the two surfaces of an electrized jar; or of the two extremities of the most extensive voltaic arrangement: and it appears to be of little consequence how thin the metallic piece may be for this development of electro-polarity: but it would be ridiculous in the extreme to refer this circumstance to the imaginary quiverings, or rotations of the metallic particles constituting the piece. I have now in my possession, some hundreds of pieces of thin zinc, each of which has had its two surfaces in opposite electrical conditions for more than ten years; but I have never attributed their electro-polarity to any quiverings or other motions of the metallic particles; nor can I conceive that such an idea is possible to be formed either from any direct fact, or even from the most remote analogy. Light certainly pervades solid transparent bodies: but it is not considered essentially necessary that the solid particles of those bodies should become agitated for its transmission: or if such intestine motions of the transparent body even were required to accommodate the theory to the fact, still they would have to be acknowledged as effects and not causes: and even the waves of light themselves, which produced those effects, would have to be traced to a still more remote cause, which itself might be discovered to be an effect of the primitive disturbing force.

14. The ticking of a watch, or the scratching of a pin at the remote end of a long piece of timber, is more distinctly heard by an ear, placed at the other end, than if no such substance had intervened; but whether the explanation of this fact were to rest on the supposition of the impressions being communicated to the ear through the medium of the air in the capillaries of the wood, or on the incomprehensible vibrations of the solid mass, the immediate causes of those movements would still have to be referred to those of the watch, or the pin: and those again to the respective forces which

* I have made more than five hundred experiments with kites for exploring the electricity of the atmosphere; and in every case, when clouds do not interfere, I have found the upper strata positively electrical, with reference to those which are below. I have had three kites, and sometimes five, at different altitudes at the same time: and have transmitted sparks from one to another from the top to the bottom of the series: in every case I found the uppermost of a pair, to be the positively electrized stratum of the atmosphere: so that if the strata in which the kites were immersed were at altitudes corresponding to the series 1, 2, 3, 4, 5, their relative electric states would be very conveniently represented by those numbers. The experiments were made at different seasons of the year, and in every part of day and night. When clouds interfere, the distribution of electricity natural to an unmolested atmosphere is often disturbed; and other phenomena occur which will be more particularly noticed in a future memoir.

† See my "Recent Experimental Researches in Electro-magnetism and Galvanism, part 1, 1830.

put these articles into motion. Hence, in these cases, as in all others, I have had occasion to notice, the phenomena of light and sound are to be attributed to extrinsic causes, there being not the slightest evidence in favour of the supposition that either class of phenomena is a mere consequence of an innate corpuscular motion of those media which immediately transmit the appropriate impressions to the respective organs of sight and hearing: and consequently no analogies discoverable, from which an idea could be formed, of any innated atomic motions of the metal being the cause of electric-polarity in the pieces of zinc already mentioned (13). Having mentioned the motions of a watch as being referrible to another cause which is obviously traceable to the main spring; and as this part of the machine appears to exert a force of its own accord, it may possibly be imagined that the hypothesis I have been discussing would find a favourable analogy in that circumstance. But it must be borne in mind, that unless the spring had been first moved by some other force it would never have exerted any power over the other works of the watch; for whilst in its original form, and unmolested, it is perfectly inert. Hence, the assumptive analogy again fails; and I believe no analogy is to be found within the precincts of physical science.

15. Whether electric phenomena be regarded as a mere variety of an extensive class, including those of light, heat, and magnetism, or as a distinct kind traceable to an individual source, the probability of an electric agent would be very great, and much favoured by analogy: but there are other sources of information of much greater importance, from which inferences may be drawn and satisfactory conclusions formed on this part of the electrical hypothesis. The indications of the existence of an electric matter are so various and extensive, that one would almost wonder how any idea to the contrary could possibly have been formed. The mechanical and calorific phenomena of electricity are those which are most usually recognised as the productions of an electric agent; although, I believe, there are none hitherto developed, that might not as easily be traced to the same cause. The displacement of granular substances; the perforation of compact bodies; and the fracture of those which are brittle, such as glass; and the violent blows given to animals by electric discharges, are all indicative of the action of some agent of considerable mechanical force. Moreover, these effects can be augmented almost to any extent, or they may be abated so as to be scarcely discernible: and this under circumstances wherein it would be said, in electrical language, that the same quantity of fluid were in motion in every case. If, for instance, the electric force excited by fifty turns of a machine, were to be collected in a high state of intensity on the surface of a jar, and afterwards discharged in the usual manner, through a metallic circuit, in an opening of which were placed a man, a pile of card paper, or a granulous substance, such as loose sand, or gunpowder; a violent blow would be given to the man, the pile of paper would be perforated or even torn to pieces, or the gunpowder would be blown away from the spot without ignition; each fact indicating a mechanical force of considerable intensity: and even of the transmission or passage of some material agent. But if, instead of the whole circuit, (unoccupied by the man, paper, or gunpowder), consisting of metal, a portion of it, amounting to five or six inches, were of a thin strip of water, or a moistened thread of cotton, silk, or any such material, no such mechanical effects would be produced. The man would experience no shock, the paper would not be torn, nor would the gunpowder be scattered as before; but it would now be set on fire.

16. It would be exceedingly difficult to reconcile these phenomena to any self vibratory motions which could be imagined to exist in the materials of the circuit, or to any motions of this kind pre-existing in the jar and transferred to them. But, by admitting the existence of an active electrical agent, distinct from the solid and liquid parts of the apparatus, we are able to find an easy and natural solution to the problems which these varied phenomena present.

17. By assuming that the fifty turns of the machine, forced a certain measure of the electric matter on to the surface of the glass, and that through the metallic circuit this matter was enabled to move with great celerity; we have then all the data necessary to satisfy the conditions of an electro-momentum of great energy, which is amply manifested by the effects it produces. But, on the other hand, when the same quantity of the electric matter is transmitted through the moistened thread, or any other inferior conductor capable of retarding its velocity, the momentum would obviously be lessened upon the strict principles of matter in motion: and the mechanical effects upon bodies placed in the circuit, would be proportionally abated; which is conformable to the results of the experiments.

18. If it can be imagined that with the metallic circuit the vibrations were more powerful than when a part of it consisted of inferior conductors, and that the mechanical action was increased accordingly, we should be under the necessity of allowing, that the less degree of vibratory motion is essential to the ignition of the gunpowder. But how should we be enabled to reconcile this latter conclusion to other facts in which calorific effects of electric discharges are so eminently displayed? A piece of steel wire is ignited by an electric discharge, under no other circumstances than when the entire circuit is metallic, or at least of very good conducting materials: and the more complet ly is the conducting powers of every other part of the circuit maintained, the more probable it is that the thin steel wire will be ignited. If the steel wire be short, it may be fused by a discharge from a moderately sized jar: but if it be long, a similar discharge would not make it visibly red hot: and by having a moistened thread in the circuit, the thin steel wire would develop no conspicuous signs of even an elevation of temperature: although, as has already been shown, the latter conditions are those alone under which the gunpowder will ignite. From these facts we are led to understand that different inflammable substances require different modes of treatment to accomplish their ignition by electric agency; whilst in a mechanical point of view, the character of the substances operated on requires no peculiarity of circuit for the production of similar effects: for invariably the mechanical action is greatest with the best conducting circuit, and abates gradually as the circuit becomes less and less perfect in its conducting character.

19. At the time I was making my experiments on the ignition of gunpowder by electric discharges, I was well aware of the necessity of varying them in every possible manner that I could think of, and the result of one of these experiments appears to have led to some doubt of the correctness of the theory which I then advanced to account for the cause of the action; the principal part of which may be embraced in a few words as follows. When the discharge is made through good conductors; as copper wire, for instance, the electric matter passes through the gunpowder with so great a velocity that it has not *time* to ignite it; but when that matter is retarded in its progress by having to traverse inferior conductors the *time* occupied to pass through any transverse section of the circuit is sufficiently great to accomplish the ignition.* This explanation has been objected to because it is a fact that in whatever part of the circuit the wet string formed a part of it, the gunpowder invariably ignited; and because the ignition was accomplished when the wet string was placed on what is usually called the *negative* side of the gunpowder, it has been thought that the string could have no part in retarding the motion of the electric matter whilst traversing the gunpowder. This objection, however, may be very easily removed by assuming the electric matter as a highly elastic fluid: and contemplating the phenomenon in question upon the principles of elastic fluids generally.

* See my papers on the ignition of gunpowder by electric discharges. London Phil. Mag. vol. lxvii; and vol. i, of the United Series of the Phil. Mag. and Annals of Philosophy.

If, for instance, a reservoir of condensed air were to be discharged through a tube sufficiently wide to offer little resistance to its motion, it would rush through the tube with considerable velocity, driving before it, of course, the air of the common density with which the tube was previously filled; or, in other words, the same quantity of air as that which was liberated from the reservoir would occupy but very little time in passing out at the farthest end of the tube. But if a similar reservoir of air were to be discharged through the same tube, now terminating with another of narrow bore, the time occupied for the escape of the air would be much greater than in the former instance; and precisely the same period of time would be occupied if the small tube were in any other part of the circuit, provided the whole of the air had to traverse it. If now this reasoning be transferred from the fluid air to the electric fluid, it will lead to similar conclusions, and show that if the velocity of the fluid be checked in any one part of the circuit it will also be checked in every other part of it.

20. That the time occupied for a discharge through the two kinds of circuits is different, being much greater in one case than in the other, is so exceedingly obvious, that no one acquainted with the experiments would attempt to deny the fact. Notwithstanding, however, it may possibly be necessary, in this place, to mention some of the appearances and effects by which it is most easily attested and understood. When the circuit is completely metallic, with the exception of a small opening in any one part for the purpose of examining the electric light, the jar is completely discharged by the shortest possible contact of the discharging rod; or, in other words, by a mere momentary closing of the remaining part of the circuit; and the electrical light at the opening, is seen but for a moment, is exceedingly brilliant, and attended with considerable noise. But when the wet thread forms a part of the circuit, the jar is not so suddenly discharged, the light is seen for a considerable time at the opening, and its former brilliancy has entirely disappeared; being now reduced to a mere redness, and attended with scarcely any noise whatever. If, whilst the circuit was metallic, a finger were placed in the opening during the discharge, the finger would receive a severe blow, and for a moment be highly illuminated within; but by employing the aqueous circuit no illumination would take place, neither would anything like a blow be experienced. The mechanical action of an electric discharge is so far abated by having a portion of the circuit of moistened thread, that the most delicate child might be placed in its way without experiencing the least inconvenience; indeed scarcely any sensation is discernible; yet in the very same circuit gunpowder might be ignited. I have frequently placed young persons in such a circuit during the discharge from six square feet of coated surface on each side the glass, charged to a high intensity. These persons never experienced any disagreeable sensations from the electric action, although eight pieces of miniature ordnance placed in other parts of the circuit have been discharged by the same electric influence. But if the circuit were to be completely metallic a similar electric discharge would produce such a violent blow that the stoutest man would not like to experience the sensation a second time.

21. In all these phenomena we have unequivocal signs of the existence of an electric matter, whose mechanical effects are as decidedly modified by varying its velocity as are those of any other species of matter whatever; and whose light also, is more and more intense as its density increases, but which becomes faint as it is attenuated, and viewed in a less compact body.

22. I am well aware that some philosophers are of opinion that the brilliant light which is developed by the electric spark, may possibly occur from a sudden displacement and subsequent collapsion of the atmospheric air, the caloric of which becoming sensible and luminous by compression; founding their reason, principally, on the fact that the air, when discharged from an air gun, pro-

duces light. Now, admitting this to be the case, no one could suppose that these effects are produced independently of an adequate force; and a physical force invariably implies the existence of active matter. But we have no idea—certainly no proof—of a piece of glass or a piece of metal being thus active; hence we are constrained to admit of the existence of some other agent in the production of these phenomena. Moreover, it is well known that although the electric light is not so brilliant in attenuated air as in that which is more dense, its existence is still manifested in a very striking and beautiful manner. Our imitations of the aurora-borealis are highly demonstrative of luminous electric matter. Besides, if there can be any discernible analogy in the light given by an air gun, and that of an electric spark, it must certainly be exceedingly remote; and the mind in which it is formed susceptible of more delicate impressions than that generally implanted in man.

23. The luminous phenomena calculated to attest the existence of an electric matter are various and extensive. The pencil, the star, the cascade, the falling star, and the electric meteor, exhibited over the surface of moist conductors, are amongst those which appear insusceptible of explanation independently of an electric matter: and the splendid bow between charcoal points attached to the poles of a voltaic battery, is untraceable to any other cause.* No one, at the present day, doubts the identity of electricity and lightning; and can there be a mind sufficiently impervious to external impressions as to doubt of lightning consisting of, or emanating from, a peculiar and active agent? And what physical agents are there which admits not of materiality, either directly or indirectly? And what known agent but the purely electric is endowed with the activity and energies of lightning? Or capable of producing those tremendous effects universally acknowledged to be attributable to this power alone? I am well aware that some philosophers are more prone to *doubt* than admit any

* There is another beautiful phenomenon of electrical light, which is not so familiar to many persons, as those mentioned in the text. It is the *luminous electro-sphere* exhibited on a positively charged conductor. The experiment is made in the following manner:—let B, fig. 1, Plate II., represent a glass receiver, furnished with a collar of leathers, and a sliding rod passing through them. The rod has a brass ball at each extremity. This receiver is to be placed on the plate of an air pump, from the centre of which rises a stout brass wire stem, surmounted with a ball. The ball *b*, is to be adjusted at about five inches distant from the ball *c*, and the air in the receiver to be attenuated by the action of the pump. If now the ball B be brought close to the prime conductor, whilst the machine is in good action, a beautiful luminous electro-sphere will be seen covering the lower side of the ball *b*; but no light on *c*. If a moveable pump plate be employed, and the whole removed from the pump, the instrument may be held in the hand by the brass cap on the top, and the plate brought close to the prime conductor. The lower ball *c*, will then have its upper surface adorned with the luminous electro-sphere; and no light will be seen on *b*. This experiment was first described by Father Beccaria, in his Treatise of Electricity, published at Turin in 1753.

I have frequently repeated this beautiful experiment, and have found that the luminous electro-sphere can be exhibited without the aid of an air pump; even in the open air. Considering, as others have done, that the pressure of the air is one of the principal causes of the electric matter being kept either within, or close to, the surface of charged conductors, it appeared likely that a partial removal of that pressure was the reason of the appearance of that matter in Beccaria's experiment; and if so, why not its appearance in the open air with a stronger electric charge in the conductor? To ascertain how far this reasoning would be sanctioned by experiment, I put my ten inch cylindrical machine in excellent order, and made the room completely dark.

The machine was kept in vigorous action by my assistant, but it was not till after some considerable time had elapsed, that I saw the electro-sphere on the ball of the prime conductor. This occurred after a great number of fine sparks had been taken from the ball; a process which I have since found conduces to the exhibition of the phenomenon; though by no means essential to its appearance.

Fig. 2, will serve to represent the remote extremity of the prime conductor, and its ball, with the luminous electro-sphere. When the machine is in good action, I never fail to see this light on the ball after an exhibition of the aurora borealis experiment, when the receiver is removed from the ball of the conductor. And, on some occasions, when no ball has been attached, I have observed a similar light partly enveloping the most remote convex surface of the prime conductor; though more frequently, this light caps a few prominences only, which stand amongst the indentations occasioned by accidental blows which that extremity of the conductor has received.

theoretical point which they themselves have had no share in establishing: and the philosophical reputation of some men rests, principally, on a steril system of *doubting*, which they gravely and inflexibly pursue. But even the most sceptic are sometimes led *indirectly* to an acknowledgment of theoretical explanations, which their proneness to doubting would not allow them directly to admit. Every philosopher who has contemplated the phenomenon traces the *immediate* cause of thunder to a sudden collapsion of displaced air. The acknowledgment of this undisputed fact admits, without further evidence, of the existence of an electric matter, which first displaced the atmospheric air: and, however reluctantly we might be inclined to concede to the fact, the long line of space from which the sound originally proceeds, constrains us to believe that the vacuum was not limited to a point, or to a small sphere of place; but that it was elongated, and produced suddenly and without interruption at very different distances from the observer: implying thereby that the matter which produced it was in very rapid motion. Nay, what observer has not seen lightning traversing a long track of atmosphere?

24. The mechanical phenomena producible by electricity are so exceedingly numerous and obviously demonstrative of the materiality of their origin, that, independently of any other, they alone afford abundant evidence of the fact. If a hard steel bar be placed vertically, or even horizontally, with its axis in the magnetic meridian, it becomes magnetic by submitting it to a violent electric discharge: but if the force of the electric discharge be so far abated, by transferring it through inferior conductors, as not to produce a sufficient degree of agitation of the steel, no such magnetic effect takes place. These effects are precisely those which would be produced by the blows of a hammer, or by any other mechanical power. Smart blows, sufficient to agitate the steel, gives an opportunity for the earth's magnetism to polarize the bar; but when the blows are feeble, no such magnetization is produced.

25. If a discharge be directed through a piece of wood, the latter will be cleft, or split into shivers. A piece of soft clay becomes disturbed, and hollow in the middle, or shattered to pieces, by a similar process. If the point of a bent wire be placed against the inner surface of a glass phial filled with oil, and sparks be taken at the other end of the wire, the glass soon becomes perforated; and many perforations may be made in a short time by moving the point to different parts of the glass, and holding the finger opposite to it on the outside. In all these instances, and in many more that might be adduced, we have direct evidence of the operations of peculiarly active matter: whose powers are still further manifested by its grinding to an impalpable powder the side of a jar, or other piece of glass, through which it has forced its way.

26. Besides the tangible, ocular, and auditory manifestations of the operations of an active subtile species of matter which appears essentially existent in the display of electrical phenomena; the olfactory and palatic organs also, bear testimony of its peculiar impressions. Every electrician is perfectly familiar with the remarkable odour developed by a machine in good action; and the peculiar tartness produced in the mouth by the application of two morsels of connected metal to the tongue, or the polar wires of a voltaic battery to the opposite sides of the face, is also well known to most persons accustomed to the use of these apparatus.

27. The former effect is producible in a variety of ways, by some of which it may be retained for a considerable time after the machine has ceased to be in motion. The room in which a machine has been working for some time, will evince electrical excitement, to any one habituated to the specific odour, for even an hour or more after the process has ended. And the aurora borealis experiment never fails to leave a strong electric odour in the receiver for a long time after it is taken from the pump plate. This latter fact is the more remarkable and important because of the odour being produced

in the vessel when nearly exhausted of atmospheric air; and appears to militate against the idea of its production from secondary causes; especially from those chemical changes which might have been supposed to have taken place in the atmospheric air; unless it can be proved that such changes are more easily accomplished when that air is much attenuated from the natural standard of density at the earth's surface. But even admitting that the smell and taste so eminently distinguishable by electric action, are secondary effects, their testimony of the entity of a primitive electric agent would be no less manifest; because the chemical changes themselves would be referrible to that agent; since neither the elements of the air nor of the saliva evinced the least tendency to such change either prior or subsequent to the electric process. Hence we discover that every organ of sense is more or less, directly or indirectly, susceptible of impressions from electrical phenomena, which transmit to the mind those special kinds of intelligence for which they are respectively and appropriately adapted: and it is by the intelligence which these impressions communicate, and by these alone, that our ideas are to be formed, our reasoning regulated, and our decisions ultimately arrived at respecting the entity or non-entity of an electric agent. Moreover, from the impressive evidence derivable from this source of intelligence, of the existence of an electric agent, and the total absence of facts, or even strict analogies from which inferences could be drawn to the contrary, we are constrained to acknowledge the entity of this agent, and to abandon the idea of accommodating electric phenomena to the indiscriminate, and hitherto undescribed motions of those bodies on which they are usually exhibited.

28. To enumerate all the facts which manifest the existence of a peculiar matter from which electrical phenomena emanate, would be an unnecessary labour for the present purpose. They would require a volume for their description, and much time for their arrangement and explanation. I have brought forward those only which appear most conspicuous to common observation, and perhaps, best known to the greater part of those persons in whose hands this memoir is likely to be placed; and, at the same time, sufficiently obvious to be understood, even by those with only a moderate degree of electrical knowledge. Moreover, as I am perfectly familiar with every fact that I have adduced, and contemplated them with much care, I labour under no apprehensions of being suspected of entire ignorance of my subject; and have no hesitation whatever, in submitting this investigation to the candid scrutiny of the ablest electrician. In every part of it there has appeared to me, full and unequivocal evidence of the existence of an electric matter; and which I have been led to believe is perfectly distinct from all others, even the *magnetic* and *calorific* not excepted.

SECTION II.

Read February 3, 1838.

" *Historical Evidence in favour of the inferences drawn in the preceding section.—Electric fluid.—Du Fay's opinion of two electric fluids.—Watson and Franklin's idea of one electric fluid, sui generis.—The author's theoretical views.—Electrical attraction.—Electrical repulsion, and its analogies in physical science.—Electro-polarization by locality.—Various explanations of this phenomenon.—Lord Stanhope's electroscopic experiments examined.*

29. The first part of this memoir has already been read before the Electrical Society, and is solely devoted to an enquiry into one of the fundamental elements of the theory of electricity: for, being the commencement of a series of memoirs which I have undertaken to bring forward, for the purpose of

conveying a comprehensive view of the various classes of electrical phenomena, and of the laws which appear to give them existence, it was deemed necessary, in the first place, to become perfectly satisfied respecting one grand theoretical particular; on which much subordinate matter seems mainly to rest: but concerning which, a very great difference of opinion has latterly been entertained by philosophers of the first degree of eminence in this branch of physics.

30. The grand controversial point, or theoretical question, will be the most intelligibly enunciated in the following manner.—Are electrical phenomena traceable to the operations of a material elementary agent, peculiar in its character, and distinct from every other species of matter? Or, can those phenomena be more easily accounted for independently of the operation of such an agent?

31. It is somewhat singular that, notwithstanding all the evidence of the best electricians that the world ever produced in favour of the entity of an electric matter, an opposite opinion should *now* be started without the slightest foundation for its support, or even one single fact in its favour. For my own part, I have so long been convinced of its existence, and founded my reasonings so completely on its operations, that nothing but a profound respect for the candour and intelligence of some of those who have speculated on the novel hypothesis, and expressed their proneness to dispense with the electric matter; and a particular desire to place before this infant society the principal facts which bear on this point, in a compact and undisguised form, that I could have been induced to devote the time necessary for their reinvestigation, arrangement, and explanation to this topic. Under these considerations, however, I have undertaken the re-perusal of many authors, and have read some others to which I had, before, not paid sufficient attention to form an opinion of their sentiments, and the value of their personal labours in this department of science. And I have also been induced to repeat several old experiments, and institute some new ones in order to satisfy myself in certain particulars, and make myself perfectly acquainted with every fact on which my reasonings have been founded. The first section of this memoir is principally devoted to these investigations: and the conclusions I have there arrived at, are the natural inferences derivable from the various circumstances connected with the phenomena already explained. I have also extended my enquiries to other electrical phenomena, both mechanical, physiological, magnetic, thermometric, and chemical: and from a rigid examination of them, and a comparison of the various ways by which some of them have been attempted to be explained, I can discover no hypothesis so free from ambiguity; none so truly specific; none so simple, distinct and comprehensive: in short, none so apparently rational and conclusive as that which admits of the agency of a purely electric matter.

32. The electric matter was recognised in a very early period of the progress of the science by some of the most active and discerning philosophers of that day. Benjamin Wilson, than whom no one was more conversant with all the then known phenomena of electricity, did not hesitate to attribute their exhibition to the operations of an electric matter; which he was disposed to assimilate to the ether of Sir Isaac Newton; although he gives it the peculiar appellation *electric matter.** Stephen Grey, who made many capital discoveries in electricity, calls this matter the "electric fire,"† which is an appellation given to it by Father Beccaria; ‡ and was also employed by Dr. Watson, who says, "that the electrical fire is truly flame, and that extremely subtile."§ Many other philosophers

* Wilson's Treatise on Electricity, 1750.

† Phil. Trans. original No. 436, p. 16; or Hutton's abridgement, Vol. VIII., p. 5.

‡ Beccaria's Treatise upon artificial Electricity, Translation 1776.

§ Phil. Trans. Original No. 471, p. 481. Hutton's abridgement, Vol. IX. p. 158.

of considerable eminence have given to the electric agent the name of electric fire: a term frequently used even at the present day; although "electric fluid" has, in a great measure superseded it. Viscount Mahon, afterwards Lord Stanhope, called it the "electric fluid;"* and Priestly also uses the same term for the electric agent.† Euler appears not so partial to an "electric fluid;" although he frequently employs the terms *positive* and *negative* electricity; and undertakes to explain the whole phenomena by the operations of an highly elastic *ether;* with which the pores of all bodies are continually charged. This ether being susceptible of compression and dilatation would assume different degrees of density accordingly as the pores of bodies were closed or expanded; which was the cause of those bodies becoming *positively* or *negatively* electrical respectively. Euler admits of the transmission of this *ether* from one body to another, and explains the spark, shock, lightning, and thunder in precisely the same manner as other philosophers have done with an "electric fluid," differing from them only in the name of the electric agent.‡ Cavallo was exceedingly cautious in adopting any hypothesis for the explanation of electrical phenomena; yet with all his diffidence, he very frankly confesses that the supposition of a purely electric fluid is certainly the most probable.§ And in another place when speaking of the *residence* of this agent, Cavallo says, "That the electric fluid, proper to a body when in its natural state, is equally diffused throughout all its substance, *I think no one will deny;*"|| which expresses this philosopher's conviction not only of the existence of an electric matter, but also of the mode of its distribution. The experiments of Du Fay led that philosopher to the belief of the existence of *two* electric fluids; independently of which he could not account for the phenomena which they presented to his notice. The hypothesis of Du Fay has been very much esteemed by the continental philosophers, and by some of them, is still held in considerable repute.

33. In an early part of the year 1747, Dr. Watson made known his ideas of the operation of *one* electric fluid *sui generis*;¶ and about the same time in America, Dr. Franklin was digesting his well known theory, which also rests upon the supposition of one electric fluid.**

34. The theory of Franklin, as far as it extends, appears to require but very little modification to become applicable to every fact that has been developed in this branch of physics prior to the discovery of electro-magnetism: and, perhaps, it is in this department of electricity alone, where the Franklinean doctrine will be found materially deficient. And even here, notwithstanding its inadequacy to account for this class of phenomena, it does not appear to be materially defective in itself, or physically incorrect: for the principles of that doctrine are as decidedly and as conspicuously in operation in this department of electricity as in any other; and by annexing the principles of electro-magnetism and magnetic electricity to those which Franklin had embraced in his theory, it is probable that we should be in possession of a code of laws, to the operation of which, every known electrical phenomenon may be traced.

35. The same mathematical formulæ are as easily deducible from the vitreous and resinous forces of Du Fay as from the positive and negative forces of Franklin; and, consequently, as applicable to

* Principles of Electricity, by Charles Viscount Mahon, 1779.

† Priestley's History of Electricity.

‡ Euler's Letters on different subjects in Natural Philosophy, addressed to a German Princess. Brewster's Translation, Vol. II.

§ Cavallo's complete Treatise on Electricity, Second Edition, p. p. 105, 114.

|| Ibid. p. 126. ¶ Priestley's History of Electricity, p. 389. ** Ibid.

the doctrine of two electric fluids as to that of one electric fluid. But it cannot be said that because mathematical processes are rigidly correct, that such circumstance confers on them the attribute of infallibility in testing the correctness or incorrectness of our notions respecting the physical agency which actuates in the production of natural scientific events. In the instance before us we have a pretty fair specimen of the *flexibility* of mathematics, which are obviously as applicable to false as to true data : for it would be an absurdity to suppose that both these theories are physically correct. The fault, however, is not in the mathematics, but in the data on which the reasoning is founded. The data once given, a moderate degree of skill would enable the mathematician to proceed in his investigation and arrive at results perfectly agreeable to the data. But should the latter be incorrect, the exactness of the investigation could have no tendency whatever to establish a true theory; for the foundation resting on no physical truth, the superstructure itself, in whatever manner it might be adorned with mathematical symbols, would be too frail to withstand those stern facts which the inflexible laws of nature develop to rigorous experimental research.

36. Mathematical investigations, however, when directed to physical operations, and based on incontrovertible facts, give a sterling value to science, and establish imperishable sources of conduciveness to the various wants of civilized life.

37. Mechanics, hydrostatics, and pneumatics, are amongst those experimental sciences whose phenomena are calculable with mathematical precision; and which, in consequence, have long become practically and extensively useful. The phenomena of those sciences requiring no dexterous hand for their exhibition, nor presenting any equivocation or capriciousness in their display, were easily reconciled to that degree of certainty and exactness which alone render experimental results susceptible of utile mathematical rule.

38. In electricity, however, the phenomena are of a very different character, presenting difficulties not to be met with in any other branch of physical research. The number of electrical phenomena is so exceedingly great as to surpass the collective sum of all others that science has revealed to man. They are exhibited under such a variety of circumstances both on masses and on the most minute portions of matter, are brought into play by such a diversity of both natural and artificial means, and viewed by experimentalists under such a dissimilarity of aspects, that it is scarcely to be wondered at that the multitude of facts which have been developed in this branch of physics, remain to this day little better than a stupendous heap of inorganized materials; intrinsically rich, but whose real value can never be justly appreciated until they have obtained a natural and obvious classification; and are reduced to some certain inflexible laws, by which they may assume not the habiliments only, but the real character of genuine systematic science.

39. From this view of the present condition of electricity, it is obvious that much still remains to be done before this branch of physics becomes sufficiently matured so as to be established within the precincts of exact science; and, perhaps, it is only from the repeated efforts of close and exact observers, who are well accustomed to the practical part of electricity, that much progress is to be expected in systemizing the phenomena to uniform scientific order; and perhaps, also, to attempt anything further than a mere step in the advancement of electricity, would be more than could reasonably be expected to be accomplished, by any individual in the present disorderly condition of the materials which are placed before him; all of which demand attention, and require the strict scrutiny of the most acute and profound electrician. I do not, therefore, in this investigation, entertain too sanguine a hope of arriving at satisfactory conclusions on every point which I may have to discuss; though I hope to be enabled to succeed in explaining some particulars which still remain

subjects of controversy and dispute. Moreover, as in this series of memoirs I shall have to notice some tributary investigations in which interesting facts have been elicited, whose explanation seems to rest on those very points about which philosophers are still at issue, it becomes essential that I make known, as early as possible, those principles upon which my theoretical reasonings are intended mainly to rest.

40. The theoretical views which I entertain of common electricity are, perhaps, not very different to those embraced in the Franklinean doctrine. I attribute electric phenomena to the agency of a peculiar species of matter, or electric fluid, whose particles mutually repel each other, but which are attractive of all other kinds of matter. By virtue of the innate repulsiveness of its particles, the electric fluid becomes highly elastic ; and is compressible and dilatable by the application, or removal, of external forces. The dilatable propensity of the electric fluid gives it a tendency to spring outwards with an equable force in every direction as from a centre ; which force is proportional to the force of compression, or to the density of the fluid. The electric fluid is transferable from one body to another ; and, like all gaseous fluids, flows with the greatest facility in the line of least resistance.

41. The atmosphere, as far as it has been explored, is continually charged with electric fluid ; and in a greater degree as we ascend from the earth's surface (13 and note). From this condition of the atmosphere it is obvious that all bodies on the earth's surface are subjected to an electric pressure, as decidedly as a solid ball of matter would be subjected to an elastic pressure if suspended in an atmosphere of gas. By this electric pressure the fluid forces itself into the pores of all bodies in proportion as they offer facilities for its admission: and as different bodies offer different degrees of facility, they necessarily become charged with it to different degrees of extent. The attractive quality also differing amongst those bodies will also be another means of their being differently electrized under the ordinary pressure.

42. Moreover, as bodies generally, in their natural condition, are compounds and not elementary, it follows that the particles of those bodies which are of a different elementary character are naturally in a different electric condition. Hence, heterogeneous bodies, however compact they may appear to common observation, are not uniformly electrized; every particle of one of the constituents being in a different electric state to every other constituent element in the compound.

43. The natural electrization of bodies is still further diversified by their mechanical structure and external polish; for a difference in the compactness of one and the same kind of matter confers on it a difference of capacity for the reception of the electric fluid. And again, the same kind of metal, though of the same compactness throughout, will have a different electric capacity by being of different degrees of polish on different parts of its surface.* Hence, by having its opposite surfaces of different degrees of polish, those surfaces will be of different capacities for the reception of the electric fluid; and will, under the common pressure, be positive and negative with regard to each other (13).† Hence, it becomes obvious that unless a body be perfectly homogeneous in itself, and of equal polish on every part of its surface, it is physically impossible that it can be equably electrized throughout; or even of uniform electric tension on every part of its surface.

44. The electric conditions of bodies in all the cases above mentioned (41, 42, 43,), are those which they would assume under the ordinary natural electric pressure, when equably distributed on

* See my Experimental Researches, Part 1, p. 64, 1830.

† Ibid, p. 72; and Annals of Electricity, Magnetism, and Chemistry, &c. Vol. I. p. 11.

every side alike; or whilst they are perfectly surrounded by the atmospheric air, and sufficiently distant from other bodies not to be influenced by the fluid they contain: for when one body is in contact with another body, since their natural electrical conditions depend upon a circumambiency of equable pressure: that equable exposure being destroyed by the plane of contact, the bodies change their electrical character, and a new equilibrium is formed, different to that which either of them assumed separately.* Hence it follows that, as the materials composing the earth lean against, or rest upon, one another, the individual masses are seldom, if ever, exposed to an equal circumambient electric pressure, and consequently are in electric conditions accordingly to their natural dispositions (41, 42, 43,) conjointly with their connexions with one another.

45. From these considerations we are led to understand that all compound particles are electro-polar, or have their opposite sides in different electrical conditions (42). And this will be the case, whether the compound be solid, liquid, or gaseous. We learn also, that bodies even of the same kind, (instance pieces of metal) become electro-polar from a variety of causes (13, 43, 44). Bodies also become electro-polar by an unequable pressure on their opposite surfaces, though not in contact with other bodies. This kind of electro-polarity arises from a local disturbance of the natural equilibrium by a vicinal superior, or inferior electric force, located on one side only, or by the influence of both forces on opposite sides at the same time. (40. 52, 54).

46. As bodies, when in contact with each other, assume an electric equilibrium as decidedly as if perfectly insulated, (44) it follows, that the bodies composing the earth's surface and the circumambient atmosphere, would constantly observe an unvarying electric equilibrium, were no physical changes to take place amongst them But as the whole are continually undergoing a change of temperature, corresponding electrical changes are as constantly going on; not only from the *direct* thermometric variations, but *indirectly*, from a variety of secondary events, or physical vicissitudes in the liquid and aerial matter; by means of which the electric fluid is scarcely ever at rest, being transported from one body to another, as their capacities for its reception alter, and giving rise to other phenomena, and various modifications of matter, unproducible by any other natural agent.

47. Notwithstanding the various degrees of susceptibility which bodies present for the reception of the electric fluid, its natural elasticity, or innate repulsiveness, appears to be essential to its intromission; unless the mutual attractions between it and other matter be regarded as the only necessary qualification for its universal presence throughout nature, and its surprising volant motions through the air, and active transiliencies among the generality of terrestrial bodies.

48. Electrical attraction is an attribute well known from the earliest period of the science, and has maintained its position in almost every hypothesis hitherto framed: and appears to be sufficiently obvious and self evident in the display of some electrical phenomena, as to be understood by every observer. The opposite force, however, has had to encounter many theoretical difficulties, which some philosophers have shown a proneness to place in its way; and is far from being generally acknowledged, as an existing principle, at the present time. Indeed, electric repulsion is totally discarded by some writers, although this force seems more essentially concerned in the production of the generality of electrical phenomena, than even attraction itself. And as some of those phenomena appear perfectly inexplicable by the attribute of attraction alone, I have not hesitated to introduce

* There is no experiment more decisive on this point than that first shown by Volta with the copper and zinc discs. Before the discs have been brought into contact with each other, each metal has its share of electric fluid due to an equable circumambient electric pressure; but when their faces are in contact, the natural pressure is removed from the joining surfaces, and a portion of fluid flows from the copper to the zinc, or in the line of least resistance (40); both metals assuming thereby, new electrical conditions.

repulsion, as a fundamental principle, into the groundwork of the theory I have advanced, considering it as a natural attribute of the electric fluid (40). And I hope to be enabled to show, not only its efficacy in the production of phenomena, but its absolute indispensableness in the explanation of some of them.

49. The repulsion attributable to electricity has many analogies in physical science. No philosopher has yet disputed the existence of magnetic repulsion, not even those who have denied the existence of a similar force in electricity; although, in many instances, the experimental evidence is as favourable in the latter as in the former class of phenomena.

50. Elasticity is a fundamental principle in the study of Pneumatics, universally acknowledged without even a dissentient opinion. But to what attribute of its particles is the elasticity of the air traceable? Why does the remaining air, in a receiver, attenuate *itself,* and fill the whole capacity after a portion has been withdrawn by the action of the pump? And why this attenuation and expansion of even the last remaining portion of air, after the most perfect exhaustion that the pump is capable of making? These are important questions to which the term "elasticity" gives no satisfactory answer. "Elasticity is expressive of no cause, signifying only the capabilities and susceptibilities of the body to which, as a convenient term, it is applied. Elasticity depends upon an innate repulsive power, which the particles of air, or other gaseous media, naturally possess, and which is indispensably essential to their existence as elements of elastic fluids. Repulsion is therefore, an atomic attribute, and the most remote physical cause to which the expansive force of air can be traced: whilst elasticity expresses both the tendency to expansion, and the susceptibility of compression, which extensive groups of the repulsive particles exhibit.

51. Now most writers on electricity have defined the electric fluid as highly elastic: and as we have seen that an innate repulsiveness of the elementary particles is obviously the primary physical cause of all fluid elasticity; this natural attribute is as applicable to the electric fluid as to the atmos-fluids. Hence, as far as elasticity is concerned, there is no more impropriety in attributing the cause to an inherent atomic repulsion in the one case than in the other. And, although we do not find condensed portions of atmos-fluids repelling one another without any apparent connexion, in the manner we observe repulsions amongst positively electrized bodies, we have ample analogy in magnetism (49); and perhaps we have no reason to expect those analogies to hold good in the infinitely grosser, and comparatively inactive atmos-fluid matter.

52. I know of no fact in which electric repulsion is more obviously the cause, than the polarization of bodies by locality (45), although there be an abundance of them which bear ample testimony of its essential influence. If a positively electrized body, P, fig. 3, Plate II., be brought to within a short distance of an insulated body, B, it is well known that the latter will become electro-polar; being negative at the extremity *n,* but positive at *p;* and neutral at about the central section, C B. Now, I would ask any philosopher who has contemplated this beautiful and highly interesting fact, with that degree of attention which it so eminently deserves, to what power he would ascribe its appearance? Certainly not to any attractions that could possibly be devised. The attraction of the body for the electric fluid at *n,* could not be lessened, nor that at *p'* augmented by the approach of the body P. We do not attribute the electrical appearances to any change that has taken place in the metal, but to a *disturbance* of the electric fluid which it contains, or which resides within it, or about it. But a disturbance implies a disturbing force; which force must have been in, or about, the body P; because the phenomenon was not exhibited prior to the location of that body: nor does it continue only during the locality of the bodies P and B. Moreover, the mere metal of the body P, did not constitute the disturbing force; because if that body had been in its natural elastic con-

dition, the polarity of B would not have happened. Hence the disturbing force proceeded from some natural attribute of the electric fluid in, or about, the body P, and not from any property of the metal itself.

Now, an attraction between the fluid in P and the metal of B, could be no means of the latter becoming electro-polar in the manner shown by the experiment: nor can it be shown that such attraction would cause any disturbance of the fluid in B, unless there were an absolute introgression of fluid to B; a circumstance neither known nor supposed to take place: for when P is removed to a sufficient distance, the body B is found in its natural inert electrical condition.

53. The difficulties presented to the explanation of this species of polarity, by rejecting electric repulsion, are entirely removed by the admission of that principle, or attribute, into the theory. Let the accumulated fluid on the body P, repel the fluid in the body B, and the explanation becomes exceedingly easy and familiar. The body B, being a good conductor, its fluid would be easily put into motion by the disturbing repulsive force on P. By this repulsive force, the fluid originally occupying the extremity *n*, of the body B, would be partially dislodged, and driven towards the extremity *p*. The extremities *n* and *p* would then be respectively negative and positive, as shown by the experiment.

54. The distribution of the fluid on B, will vary accordingly to the extent of disturbance; which may be made to vary either by varying the electric power of P, or by varying the distance between the two bodies. Hence it is obvious that the position of the neutral plane B C, will change its position accordingly with these circumstances. The neutral part of B can hardly be defined as a plane in all cases; because, when the conductor B is large, there is a neutral zone of some considerable dimensions, whose position will vary with the circumstances connected with the disturbance.

55. If, instead of being located with the positively electrized body P, the conductor B, were in the vicinity of a negatively electrized body N, fig. 4, the character of polarization in B would be the reverse of that in the former case. Or the nearest extremity *p* would be positively, and the remote extremity *n*, negatively electrical. To explain this event we have only to understand that the natural electric pressure (41) on the extremity *p* is lessened by the presence of the negative body. The fluid in B will now obey the law observable in all fluids, and will move in the line of least resistance, or, towards the extremity *p*, and thus the body B will become electro-polar. When the negative body N is sufficiently removed, the fluid of the body B will again experience an equable circumambient pressure (41), and will consequently resume its former natural distribution.

56. The experiment which I have been describing has been known to electricians for many years,* and the explanation which I have given of it, is far from being new. It is, in principle, the same as is given by several authors; but perhaps the most minutely described by Viscount Mahon, afterwards Lord Stanhope; by whom the greatest variety of experiments were made on loco-electrization that are on record.† I have repeated most of this Nobleman's experiments on loco-electrization,

* Otto Guericke, a famous philosopher, and Burgomaster of Magdeburg, who flourished about 1670, was the first to discover that bodies could be electrized without having the fluid communicated to them. Priestley's History of Electricity, p. 8. Some experiments by Stephen Grey also showed electrization by locality. This ingenious electrician, in the year 1730 electrized a boy, suspended by silken cords, by bringing near to him an excited glass tube. When the tube was brought near his feet the greatest action was about his head. Phil. Trans. Original No. 417, p. 81. Hutton's Abridgement Vol. VII. p. 459. Priestley's History of Electricity, p. 32. But Canton, Franklin, Æpinus, and Wilcke, made the first interesting series of experiments on this branch of electricity. Priestley's History of Electricity, p. 211.

† Principles of Electricity. By Viscount Mahon, Quarto. 1779.

and have found his descriptions of the phenomena exceedingly correct; although in some others he appears to have been very much deceived. Beccaria also made many interesting enquiries in this class of phenomena,* which I shall have more particularly to notice in another place. Dr. Milner, of Maidstone, also made many exceedingly interesting experiments in loco-electrization, which are diversified in a great variety of ways.† I am well aware that Lord Stanhope's explanation of these phenomena has not been generally received as orthodox electricity, although it is by far the simplest and most intelligible that has been proposed: and in every other attempt at explanation the principle of *repulsion* is obviously resorted to.

57. Mr. Morgan has attempted to repudiate Viscount Mahon's theory with greater determination than, perhaps, any other writer on this subject; but his views of different phenomena are not very consistent with each other; and with reference to the present question, not very satisfactory. Whilst speaking of the polarized body, Mr. Morgan says, in rather a lofty tone, "a metallic body is said to be in two different states at the same time. What single electrical fact is there to warrant this assertion? What is there intelligible in electricity, if we admit that perfect conductors can have their equilibrium disturbed, or that two different parts of them can be at the same time in two different states, when there is no kind of insulation to separate the positive from the negative, but, on the contrary, such a communication, as in every other instance, immediately restores the equilibrium?"‡ Perhaps whilst Mr. Morgan was writing this sentence he had forgotten that he had previously shown that a continuous conductor could be both positive and negative at the same time. Whilst describing the electric conditions of the coatings of one jar and the lining of another, in metallic connexion with each other, he says, "the deficiency is in connexion with the superabundance".§

58. Those electricians who have written in favour of Du Fay's hypothesis of vitreous and resinous fluids, explain the phenomenon (51) upon the principle of decomposition. The vitreous fluid of the body P, fig. 3, attracts the resinous fluid of the conductor B; and at the same time *repels* the vitreous fluid of B to the most remote extremity *p*. When the conductor B is located with a negatively electrized body N, fig. 4 (54), the resinous fluid of the body N is supposed to attract the vitreous fluid of the conductor B, and draw it towards the nearest extremity *p*; whilst, at the same time, the resinous fluid of N repels the resinous fluid of the conductor B, to the remote extremity *n*. Thus, in both cases, *repulsion* is required to explain the phenomena. The same principles are here applied to *both* fluids, as Franklin gave to his *one* fluid; and as by the latter the problem is easily solved, there can be no philosophical propriety in admitting the compound fluid of Du Fay.

59. The phenomenon which appears to present the greatest impediment to the universal reception of electrical repulsion, is the separation of light bodies when negatively electrized. The hypothesis of Du Fay admitting of electric repulsion in both its fluids, is perfectly prepared for the explanation of this phenomenon; by alluding it to the natural attribute, common to the vitreous and resinous fluids; which, although attractive of each other, are individually repulsive of themselves (57). Hence, the difficulty in solving the problem which this phenomenon presents is limited to those who admit of one electric fluid only; some of whom have had recourse to the supposition, that

* *Giambattista Baccaria* dell' electricismo artificiale e naturale. 1757, Turin, Quarto. English Translation, sec. 3. 1776.

† Experiments and observations in Electricity. By Thomas Milner, M. D. 1794.

‡ Morgan's Lectures on Electricity, Vol. II. p. 275. 1794.

§ Morgan's Lectures on Electricity, Vol. I. p. 110.

all matter is repulsive of itself; and that bodies which are negatively electric exhibit this supposed universal attribute more perfectly than when in any other condition. This method of explaining the phenomenon was first proposed by M. Æpinus,* and afterwards adopted by Mr. Wilcke,† and still further supported by the Honourable Mr. Cavendish, who made an immense number of experiments, with some mathematical investigations, which to him appeared favourable to that hypothesis.‡ The same views of negatively electrized bodies are still favoured by some more modern writers on this subject.

60. To me, there appears no necessity to load the theory of electricity with this auxiliary force; because other methods of solving the problem are quite as satisfactory. Let the two balls, *x y*, fig. 5, be attached to a negatively electrized body B. Now the balls being deprived of their natural share of fluid, will endeavour to recover it again from the nearest portions of the surrounding air; and in consequence of both of them drawing a supply, at the same time, from that plate of air directly between them, it will become more negative than any other stratum in the vicinity of the balls, and consequently less enabled to continue the supply. This being accomplished, and the balls giving their newly acquired fluid to the body B as fast as they collect it, they will remain negatively electric. There will now be an attraction exercised between these balls and the electric fluid of the atmosphere; and as the balls yield this fluid to the body B more freely than it is capable of extricating itself from the distant particles of the air, the balls travel in quest of new supplies towards those places where it is most abundant, or *from* the stratum which they have already partially deprived of its electric fluid. Hence it is that the balls move in opposite directions from their original plane of contact, and by their divergency appear to repel one another; although, it is probable that the phenomenon does not essentially depend upon repulsion, but is principally the consequence of electrical attractions.

61. Some experiments related by Earl Stanhope have been the cause of more theoretical speculation on this particular topic, than any other with which I am acquainted; and, like some of more modern date, have been productive of very much delusion. The experiments and the inferences drawn from them, are described in the following manner:—

" *a. Experiment* 1, *fig.* 6. I took a pair of cork-ball electrometers A B, whose balls were three-eighths of an inch diameter, and whose *parallel legs* were eight inches long; and I suspended them to a hook that was fixed to the underside of the brass cap *c d* of the glass receiver E, F, G, H, of an air-pump, as shown in the figure. The two legs of this electrometer, in order to prevent their twisting, were made of fine straws which had been previously well soaked in salt and water, to make them conduct better. Each of these legs was suspended at top, by a very fine linen thread of about *one-twelfth* of an inch in length.

" *b.* In order to render the glass receiver a good *non-conductor* of electricity, I caused it to be perfectly well dried by means of fire. It was proper, in this experiment, to avoid giving any friction to the glass receiver, for fear of charging the glass.

" *c.* Upon charging a small prime conductor, which was made to communicate with the brass cap of the glass receiver, the electrometrical balls divaricated above *two inches and a half.*

* Tentamen Theoriæ Electricitatis et Magnetismi, Petersburg, 1759. Priestley's History of Electricity, p. 395.

† Ibid. p. 396.

‡ Phil. Trans. Hutton's Abridgement, Vol. XIII. p. 223.

"*d. Experiment* 2. I then began to *exhaust* the receiver; upon doing which, the balls soon began to devaricate *less and less.* And as soon as the short barometer gage was got down to about *one quarter* of an inch (the barometer being, that day, at the height of *twenty-nine inches and a quarter*); the devarication of the balls from each other, became reduced to *less* than *one quarter* of an inch.

"*e.* So that, by $\frac{116}{117}$ parts of the natural quantity of air contained in the receiver, being *exhausted,* the devarication of the electrometrical balls was diminished to less than *one tenth* part. For, the *chord* of the angle of divarication was decreased (as was said before, *c* and *d*) from above *two inches and a half* to less than *one quarter* of an inch. That is to say, that the *versed sine* of the angle of devarication, was decreased considerably more than an *hundred times*; because, *the versed sines are always as the squares of the chords.*

"*f.* I should be inclined to imagine, if this experiment were made with great accuracy, and with a proper electrometer, that the *versed sine* of the angle of devarication, would always be in the *same ratio,* as the density of the air in the receiver; provided that proper means were taken to keep the apparatus sufficiently free from moisture during the experiment.

"*g. Experiment* 3. I then electrified the glass of the receiver itself; but as long as the receiver remained *exhausted* to the degree above mentioned (*d*), it was out of my power to cause the electrometrical balls to devaricate above *one quarter* of an inch.

"*h. Experiment* 4. I then let the air return into the receiver, which circumstance alone caused the balls again to divaricate considerably, although I gave to the apparatus *no fresh supply of electricity.* But the balls took up a short time in coming to their full degree of devarication. The reason of which, evidently was, that the *unelectrified* air, which entered into the receiver, could not receive *immediately,* from the charged apparatus, that degree of electricity which it was able finally to acquire.

"*i.* I repeated these experiments with the exhausting air pump, several times, and I always found results that were similar.

"*j.* From these experiments, it appears, that, when bodies are charged with electricity, it is the *particles of (circumambient) air being electrified,* that constitutes the *electric atmosphere* which exists around those bodies.

"*k.* Now, since an *electrical atmosphere* (whether *negative* or *positive*), consists of electrified *air,* it evidently follows, that the density of the electricity of the air, must be in some *inverse ratio of the distance* from the charged body, which causes that electric atmosphere.

"*l.* That electric atmospheres do decrease *in density,* the more the distance from the electrified body is increased, is demonstrable by means of a proper electrometer in every instance.

"*m.* From these simple considerations, it is easy to reduce, all the different *phenomena* of electrical *attraction* and *repulsion* to *one* plain and convenient principle, derived from the very nature of a *disturbed electrical equilibrium*; namely, to the elastic tendency of the electric fluid, to impel every body, charged either *in plus* or *in minus,* towards *that part* of its electric atmosphere, where its *natural electrical equilibrium* would be the most easily restored.

"*n.* From this simple principle, it is evident, that bodies, which are charged with contrary electricities, must tend to *approach* each other, whenever the skirts of their (oppositely electrified) *atmospheres* interfere.

"*o.* From the same simple principle, it is also easy to understand, why bodies, that are charged with the *same kind* of electricity, tend to diverge from each other.

" Every body that is electrified (whether *in plus* or *in minus*) has a constant tendency to return to

its *natural state;* and this causes it to electrify, in a certain degree, *other bodies* in contact with it, and the *air* in its vicinity, in a manner similar to that explained above.

"*p.* If two bodies (for example) be both *positive;* neither body will be able to deposit its *superabundant* electricity upon the other body, which is also similarly electrified *in plus.* It is therefore evident, from the simple principle mentioned above (*m*), that if these bodies be brought near each other, *each body* will be impelled, towards the particles of air on its *other* side, which are electrified *in plus* only in a *small* degree. That is to say, that each body will tend to *diverge* from the other.

"*q.* If these bodies, on the contrary, be both *negative;* neither body will be able to have its *deficient* electricity supplied from the other body, which is also similarly electrified *in minus.* It is therefore evident, from the the simple principle mentioned above (*m*), that, if these bodies be brought near each other, *each body* will be impelled, towards the particles of air on its *other* side, which are electrified *in minus* only in a *small* degree. That is to say, that each body will tend to *diverge* from the other.

"*r.* So that, bodies, which are charged with the *same kind* of electricity (whether *positive* or *negative*), must necessarily tend to *diverge* from each other."

62. The inferences which the noble author has drawn from his experiments are obviously at variance with the doctrine of electrical repulsion; which is the more remarkable because he has acknowledged the *elasticity* of the electric fluid (*m*); a property evidently traceable to the attribute of repulsion exercised by its individual particles (50, 51).

63. There can be no objections to Lord Stanhope's method of explaining the phenomena, as far as it proceeds; because attraction is unquestionably in play in the divergency of similarly electrized bodies, as that phenomenon is usually displayed: but neither the experiments of that nobleman, nor the inferences he has drawn from them, have any tendency whatever to disprove the existence of a repulsive electric force.

64. I am well aware that these experiments are usually looked upon as master-pieces of their kind, and are much admired and frequently quoted by those philosophers whose opinions are hostile to the doctrine of electrical repulsion; and as their correctness has never yet been disputed, they are regarded as affording *standard* data, on which much theoretical speculation has been founded.

65. To me, however, these experiments have never appeared in that light; but, on the contrary, I have always considered the data which they afford, much too scanty, if even the *recorded* results had been admissible as facts on which implicit confidence could have been placed. And on looking at the circumstances connected with the experiments, it is not difficult to perceive that those results are placed in a very questionable posture; and are obviously objectionable in whatever point of view the scientific electrician may contemplate them.

66. The electrometer (60. *a.*), which Lord Stanhope employed in these experiments, was not much calculated to give very exact results in an atmosphere so far attenuated as that it is said to have been placed in (60. *d.*). Its balls were far too heavy (60. *a.*) to be kept divergent by any electric force which they could retain in so good a conducting medium as that of an atmosphere supporting only one quarter of an inch of mercury. The electric force which kept the balls two inches and a quarter apart, (60. *c.*) in a common atmosphere, would be mostly lost in the attenuated one, (60. *d.*); for withdrawing the air with the pump, would remove the insulation; and consequently a portion of the fluid would make its escape from the straws and their balls; probably to the base of the instrument. But it is stated by Lord Stanhope that the electric force was *not* lost by the

attenuation of the air; for he says that when the air was re-admitted, the balls again devaricated "considerably, although I gave to the apparatus *no fresh supply of electricity*" (60. *h.*): and from the subsequent part of that paragraph we are led to understand that the balls ultimately diverged to the same extent as at first; a conclusion not very consistent with the doctrine of electric atmospheres (60. *k.* to *r.*). For, one would be led to suppose that the first electric atmospheres of the balls would be partly removed by the action of the pump, and another pair have to be formed on the re-admission of the air, and the formation of these second atmospheres would have to be at the expense of the electric fluid from the balls, straws, and cap of the instrument, in all of which, the remaining fluid would be attenuated, and the divergency ought not to be so great as before.

67. I do not know that Lord Stanhope's experiments have ever been repeated; or if they have, I should suppose, from the estimation in which they are held by some of our latest writers on this subject, that they have never been much varied; and but very imperfectly understood. Singer, who has left the best treatise on electricity, in the English language, speaks of Lord Stanhope's experiments with great confidence: and quotes them in support of his opinion of there being no such attribute as electric repulsion.* Since the publication of Singer's work, I cannot find that any scientific journal has noticed these celebrated experiments farther than an occasional quotation; and therefore I am in hopes that the experiments which I am about to detail, will appear interesting, not only to the London Electrical Society, but to philosophers generally who are engaged in theoretical enquiries in this branch of physics.

68. I prepared an electrometer similar to that employed by Lord Stanhope (60. *a.*); and proceeded to repeat the experiments in the manner already described (60. *c, d, h,*). Prior to the attenuation of the air in the receiver, the cork balls were caused to diverge to about two inches from each other by the application of an excited glass tube. The pump was immediately brought into requisition, and whilst the air in the receiver was being attenuated, the divergency of the cork balls began to be lessened; and before the attenuated air was counterbalanced by one inch of mercury, the balls got to within a quarter of an inch of each other. The air was now readmitted to the receiver very slowly; but the balls showed no tendency to separate again. I repeated this experiment many times with similar results.

69. It now occurred to me that the cork balls were too heavy; and that, relatively to the two *extreme* conditions of the air (dense and rarefied) in which they were immersed, they would be heavier in one case than the other. This latter circumstance, however, could not prevent the balls from diverging again when the air was readmitted; for if the electric force had continued unimpaired till the return of the air, the divergency ought to have progressed as the density was restored.

70. I now removed the cork balls from the ends of the straws, and replaced them by small balls of the pith of the elder. I again electrized the apparatus with an excited glass tube until the balls diverged to about two inches from each other. On attenuating the air to the same extent as before, the balls approached to within about 3-8ths of an inch of each other. The air was readmitted gently, but the balls never separated any farther than whilst the air was attenuated. This experiment I also repeated several times and the results were always of a similar description; the collapsion of the balls usually bringing them to *within* half an inch from each other when the air to which they were exposed counterbalanced one inch of mercury; but in no case did they diverge again when

* Elements of Electricity and Electro-chemistry. By George John Singer, p. 24.—London, 1814.

the air was restored to its usual density. From these facts nothing seemed more likely than that a portion of the electric fluid had escaped by the attenuation of the air in the receiver.

71. My next experiment was intended to ascertain if the pith balls could be made to diverge to two inches in air attenuated so as to counterbalance only one inch of mercury. The air in the receiver having been brought to this standard density, the excited glass tube was brought to the cap of the instrument. The straws and balls now exhibited some strange antic motions not easily described. They would first suddenly diverge to a considerable extent, and as suddenly return to their vertical position in the axis of the glass; repeating these motions two or three times before the tube came into actual contact with the cap of the electrometer. And it was often with great difficulty that they could be made to remain separate when the excited tube was taken away. From the results of this experiment, often repeated, it seemed obvious that the fluid was given off by the straws and balls, either to the sides of the receiver, or to the pump-plate. The latter, however, appearing the more probable course for it to take, I contrived the following experiment to ascertain how far this view might be correct.

72. Two electroscopes were provided for this experiment, which, when properly prepared, were placed the one on the other, as represented by fig. 7. The receiver A, prior to its being situated on B as in the figure, was placed on the plate of a jet d'eau experiment apparatus, whilst the latter was screwed in the orifice of the pump plate. The air in A was then attenuated until it would just counterbalance one inch of mercury. This done, the communication was cut off by turning the stop-cock *s*, the apparatus was then taken from the pump, and screwed to its base *d*, and afterwards placed on the electroscope B, whose contained air was of the density of the atmosphere. On bringing the excited glass tube near to the cap of the electroscope A, the pith balls made several singular motions, but did not evince much tendency to diverge from each other. But the pith balls within B, diverged to an inch and a half at least, as decidedly as if the tube had been brought *directly* to its cap. When the excited tube was taken away, the balls in A hung close together; but those in B remained divergent, as shown by the figure. The tube was excited anew, and again applied to the cap of A, whose balls again were much agitated, and the balls within B diverged farther than before, and even struck the sides of the glass. By a few trials I found that any degree of divergency might be given to the balls in B; but that there was great difficulty in keeping those in A separated more than about half an inch from each other after the excited tube was taken away: although, by regulating the distance between the tube and the cap of the instrument, they might be made to separate one inch or more.

73. The experiments (71) were repeated, by applying a large piece of excited amber, instead of the glass tube, to the cap of the instrument A. The results were similar to those obtained when the glass tube was employed, only the balls in A were not so much agitated. The balls in B, separated as before, though not to so great an extent; whilst those in A, separated farther than by the application of the tube; and remained more divergent after the amber had been taken away. The balls in B were made to separate two inches and a half, by three applications of the excited amber to the cap of A; and remained separated for some time, after the amber was removed.

74. Now, although the results of these experiments appeared satisfactory enough that the fluid communicated to the instrument A, from the glass tube, was transmitted through the attenuated air to the lower instrument B; and when the amber was used the fluid moved in the opposite direction: yet it was necessary to ascertain how far the two instruments would be affected by electrical locality alone (52), when both were filled with air of the common density of the atmosphere, which at that time counterbalanced 29·6 inches of mercury; the temperature of the room being 60° F. The

two instruments were again placed as in fig. 8, and the excited glass tube made to approach the cap of A. The balls in this instrument diverged considerably; but those in B were scarcely affected. On bringing the tube into contact with the cap of A, the pith balls struck the side of the receiver; and those in B, separated about half an inch; and remained about a quarter of an inch apart after the excited tube had been taken away. The experiment was often repeated and with similar results: excepting that the balls in B did not *always* remain divergent after the excited body was removed from the cap of A: but generally remained as in the figure.

75. By comparing the results of the above described experiments (71, 72, 73,) we discover a material difference between those which were produced when the air in the instrument A, was attenuated, and when at the common density of the atmosphere. When the balls were immersed in the attenuated air (71, 72,), their motions were rapid and exceedingly irregular; unless great care were observed in bringing the excited body very slowly towards the cap of the instrument. And even when the greatest care was taken, the balls would suddenly strike each other after a moment's separation: and would repeat these motions two, three, and often four times before the excited tube arrived at the cap of the instrument. But when the air within the instrument A, was of the common atmospheric density, no such vacillancy was exhibited by the balls (73). The divergency was invariably regular, and progressive as the excited body approached the cap of the instrument: and in no case did the balls strike each other whilst under the electric influence of the tube, or the amber. Another striking contrast in the results of these experiments is observable in the *ultimate maximum* divergency* of the balls in the two instruments. When the air is attenuated in A, the *ultimate maximum* divergency is invariably *greater* in B than in A (71). But when the air in both instruments is of the common atmospheric density, the *ultimate maximum* divergency is uniformly *greater* in A than in B (73). By taking into consideration every circumstance connected with the above experiments, there was every reason to suppose that when the air was attenuated in the instrument A, it became a sufficiently good conductor to carry off a portion of the fluid from the cap, straws, and balls of the instrument: and that this fluid was transmitted to the lower instrument B.

76. Thinking that, if, in place of the salted straws, two better conducting stems were suspended in the instrument A, the loss of fluid in an attenuated medium would be better observed, I procured, for this purpose, some fine copper wire, from which proper lengths were taken. One extremity of each piece was bent into the shape of a hook, and the other extremity furnished with a pith ball; and both wires were hung in the axis of the receiver. The instrument was now placed on the plate of the air pump; and before any attenuation was carried on, the excited glass tube was made to approach the cap of the electroscope. The balls separated from each other as gradually as when attached to the straws in the former experiments, in air of the common atmospheric density: and when the tube was brought close to the cap of the instrument, the balls struck the sides of the receiver. When the electric force of the tube was not too great, the divergency might be extended to any required degree without the balls striking the receiver: and if the cap were touched by the tube, the balls would remain two inches apart for several minutes after the tube was taken away. To ascertain this latter fact, was the principal object of this experiment.

* "*Ultimate maximum* divergency" is intended to express the divergencies in their last stages; or when the excited body is withdrawn from the instrument A, which is the only period of the experiment in which a just estimate of their relative extent of divergencies can be formed. For, although the extent of divergency of the balls in B may very easily be ascertained in any stage of the process, the sudden vacillancy of those in A, precludes the possibility of knowing to what extent their earliest divergencies are carried.

77. The instrument being deprived of its electricity acquired in the last experiment, the pump was brought into play until the attenuated air in the receiver would just counterbalance one inch of mercury. On approaching the cap of the receiver with the excited glass tube, the wires with their balls were strangely disturbed: their motions being more rapid and frequent than those exhibited by the straws; but remained close together when the tube was taken away. By a few trials, I got into the method of leaving the balls separated about half an inch from each other; but in no instance could I obtain an *ultimate maximum* divergency to a greater extent: and even this only for a few moments after the excited body was withdrawn, for the balls soon came down to less than a quarter of an inch from each other.

78. I now varied the experiment by first electrizing the wires and balls, and afterwards attenuating the air, as had been done whilst the straws were suspended in the receiver (67, 69). The instrument being placed on the pump-plate, the balls were made to diverge by the application of the excited glass tube. When the tube was removed from the cap of the receiver, the balls were about two inches apart. The pump was now brought into play, and as the attenuation of the air proceeded, the balls came closer together; and when the mercury in the gage was reduced to one inch, the balls were less than one quarter of an inch apart. The air was readmitted very gently, but the balls never separated farther than when in the attenuated air.

79. I have repeated every experiment herein described, many times over, and have taken every care that I could think of to prevent error in the results. The experiments described (in 67, 69, 77), are those alone which can be considered as repetitions of that on which so much theorizing speculation has been ventured; and as the experiment requires no very refined experimental dexterity, it is not difficult to discover that there is some unaccountable error in Lord Stanhope's description of the results (60, *h*); for in no instance have I yet seen the least tendency to divergency of the balls by readmitting the air into the receiver; nor indeed can I see any cause for the appearance of such a phenomenon, unless it were from the greater degree of buoyancy which the balls would experience in the dense than in the rarefied air.

80. The experiments detailed (in 70, 71, 72, 73, 76), may be regarded as perfectly original, and such as the nature of the enquiry obviously required. In every one of these there has appeared, almost, indubitable proof of a *loss* of fluid through the medium of the attenuated air; a conclusion which will be strongly corroborated by the next described experiments.

81. The electroscope, with its wire indices and balls, was placed on the pump-plate, and the balls made to diverge, sometimes by the application of the excited glass tube, and at others, by the excited amber; the air in the receiver not being molested by the pump. The standard divergency was two inches, when the exciting body had been taken away. The object of the experiment was to ascertain whether the balls, when electrized to the same extent, would retain their divergency for a longer period when the air was undisturbed, or when it was attenuated by the pump. The temperature of the air of the room in which these experiments were made was 60° F., and the barometer stood at 29·6 inches. The results of the experiments are shown by the following table. The left-hand column shows the standard distance of the balls at the commencement of each observation; and the character of the excited body employed. The second column shows the time required for the loss of the standard quantity of electric action, when the balls remained in an atmosphere counterbalancing 29·6 inches of mercury. And the third or right hand column shows the time required for the same loss of electric action when the air about the balls was reduced to a pressure equal to that of one inch of mercury.

Table of experiments exhibiting the time in which an electrometer lost a standard quantity of electric action, in aerial media of different densities.

The standard repulsive distance of the pith balls, 2 inches, when excited by	Time required for the total loss of the standard quantity of electric action.	
	In air balancing 29·6 inches of mercury.	In air balancing 1 inch of mercury.
Glass	5 minutes.	$1\frac{1}{2}$ minutes.
Amber	4 minutes.	$1\frac{1}{4}$ minutes.

82. Having now satisfied myself that the *lessening* of the divergency of the balls when the air in the receiver became attenuated (60. *d,* 67, 69, 70, 71, 72, 76, 77, 80), was owing to a real loss of the electric fluid which they sustained; and that the total disappearance of the standard quantity was much facilitated when the air in which the balls were immersed was attenuated (80), I now thought it possible that the total disappearance of the electric action on the balls might be effected in a still less period of time than that shown in the table (80), by *diluting* the electric fluid, with which the instrument was charged, with fresh portions of air. For this purpose the receiver was made quite dry and warm: and when placed on the pump-plate an electric charge was given to the balls from the excited glass tube; which caused *an ultimate maximum* divergency of two inches and a quarter. This being accomplished, the apparatus was permitted to remain, unmolested, till the divergency entirely disappeared: which occupied *six minutes and a quarter.* The instrument was now charged again to the same standard of divergency as before. As soon as the glass tube could be got out of the hand, the pump was brought into play, and the mercurial gage brought down to three inches. The air was readmitted; and again pumped out until the balls came close together. The air was now again readmitted very slowly, but the balls did not diverge again: so that by this one *dilution* of the electric fluid, the whole of its action on the balls entirely disappeared. The time occupied was *forty-seven seconds.*

83. It had occurred to me, at various times during these experiments, that there was a probability of even *seeing* the electric fluid make its escape from the balls through the attenuated air in the receiver, provided the room were darkened; but, being at that time otherwise engaged in the evenings, a considerable period elapsed before I had an opportunity of ascertaining the correctness or incorrectness of this idea. Eventually, however, the experiment was made, and with the anticipated success. The electroscope with the wire indices (75) was placed on the pump-plate, and the air within attenuated till it would just counterbalance one inch of mercury. The glass tube was then excited and brought towards the cap of the instrument: and the wires and balls were agitated in the manner already described (76). The room was now darkened and the tube again excited; and then brought to the cap of the electroscope. Sparks immediately appeared from both balls, darting in a very beautiful manner to the sides of the receiver at nearly the same height as the balls were suspended: and from these places exhibited luminous streaks down the sides of the glass to the plate of the pump. This beautiful and conclusive experiment I was induced to repeat many times: during which I

frequently observed three, and sometimes four of these streaks of electrical light by one application of the excited tube. The streaks of light exhibited in this experiment, are tolerably represented by the crooked lines *o b, o b, o c, o c,* in fig. 9.

84. When excited amber was employed instead of the glass tube, the light was seen in the receiver as decidedly as in the last experiment (82); but its appearance was very different, being much fainter and not in such well defined lines. Both of the pith balls appeared beautifully illuminated, especially on their lower sides. I tried to vary the light by placing pointed wires on the pump-plate, as represented by *w w,* fig. 10. By this means the light about the balls became a little brighter than before, and extended farther from them, always inclining towards the points *w w;* but in no instance was there much brilliancy, nor any defined streaks of light, as when the glass tube was used. The figure of each portion of light had some resemblance to an inverted cone, as represented at *b c,* fig. 10, and did not appear very unlike the tail which some comets have exhibited.

85. It may here be proper to state, that I have invariably obtained more accurate results from the metallic wire indices, or stems (75), of the electroscope, than from the straws soaked in a solution of common salt (60 *a*). With the former, the loss of fluid is regular and uniformly the same in every experiment made under similar circumstances: but with the latter the results are much influenced by the hygrometric condition of the salt, and also by the asperous surfaces which it gives to the straws. Dry straws which are not salted give different results to those which are so treated: and gold leaves give still different results to either straws or wires. These and many other circumstances connected with electrometric experiments in air of different degrees of density, and some novel facts, will be more particularly noticed in my second memoir, which will shortly be submitted to the consideration of the Electrical Society. As far as I have hitherto proceeded, there have not appeared any facts which militate against the operations of a repulsive power in the display of electrical phenomena: but on the contrary, there is much evidence in favour of the existence of that attribute. It will have been observed that Lord Stanhope's experiments are totally inconclusive on this point: and it is somewhat remarkable that his Lordship, who had made so many excellent experiments on the polarization of bodies by electrical locality, should, in this instance, have confided so much on one single experiment, without even the slightest variation: for his Lordship says, in the appendix to his work, at page 235, that those "experiments were performed with *positive* electricity only." And again at page 236, "It was quite unnecessary, to make any similar experiment with *negative* electricity." From these statements and from the great confidence which Lord Stanhope placed on this single experiment, it is obvious that his Lordship had not the slightest suspicion of the escape of fluid in the attenuated air: and from the implicit sanction which this experiment has generally met with amongst philosophers, it would seem that not the remotest idea of the fact has hitherto been entertained.

86. It will have been observed that in none of the experiments hitherto described have I attenuated the air in the receiver to a greater extent than as a counterbalance to one inch of mercury: whereas, in Lord Stanhope's experiment, the density of the air was reduced so as to counterbalance only one quarter of an inch of mercury, (60. *d.*). I have adhered to the former standard density of the air in the receiver on two different accounts. First, because when the mercury in the *short* barometer gage had fallen to one inch, the cork balls had approached to about a quarter of an inch of each other (67); which is the *shortest* distance between the cork balls in his Lordship's experiments (60. *d.*). Secondly, as the generality of pumps which are in the hands of experimenters, will reduce the mercurial column to one inch, and but only a few of them sufficiently accurate to bring it down to a

quarter of an inch, I have thought it better to record the experiments under those circumstances in which they may be repeated with the greatest facility; and within the range of those means which are at command by the greatest number of experimenters. It will be necessary to mention, however, that I have attenuated the air about the electrized balls to a much greater extent: but, as might have been expected, the loss of the fluid was greater as the mercurial column, in the gage, shortened: and I have often found, that, when the air was counterbalanced by less than half an inch of mercury, the balls would come down to one tenth of an inch from each other. Much exactitude, however, will always be required in experiments of this kind; for, as the loss of the electric fluid will depend both on the *attenuation* of the air and on the *time* occupied in pumping (80. 81.), it is obvious that the divergency will be lessened on both these accounts, and the *distance* between the balls when the mercurial column is reduced to any *standard altitude*, will depend upon the *time* occupied in bringing the air to the given degree of attenuation.

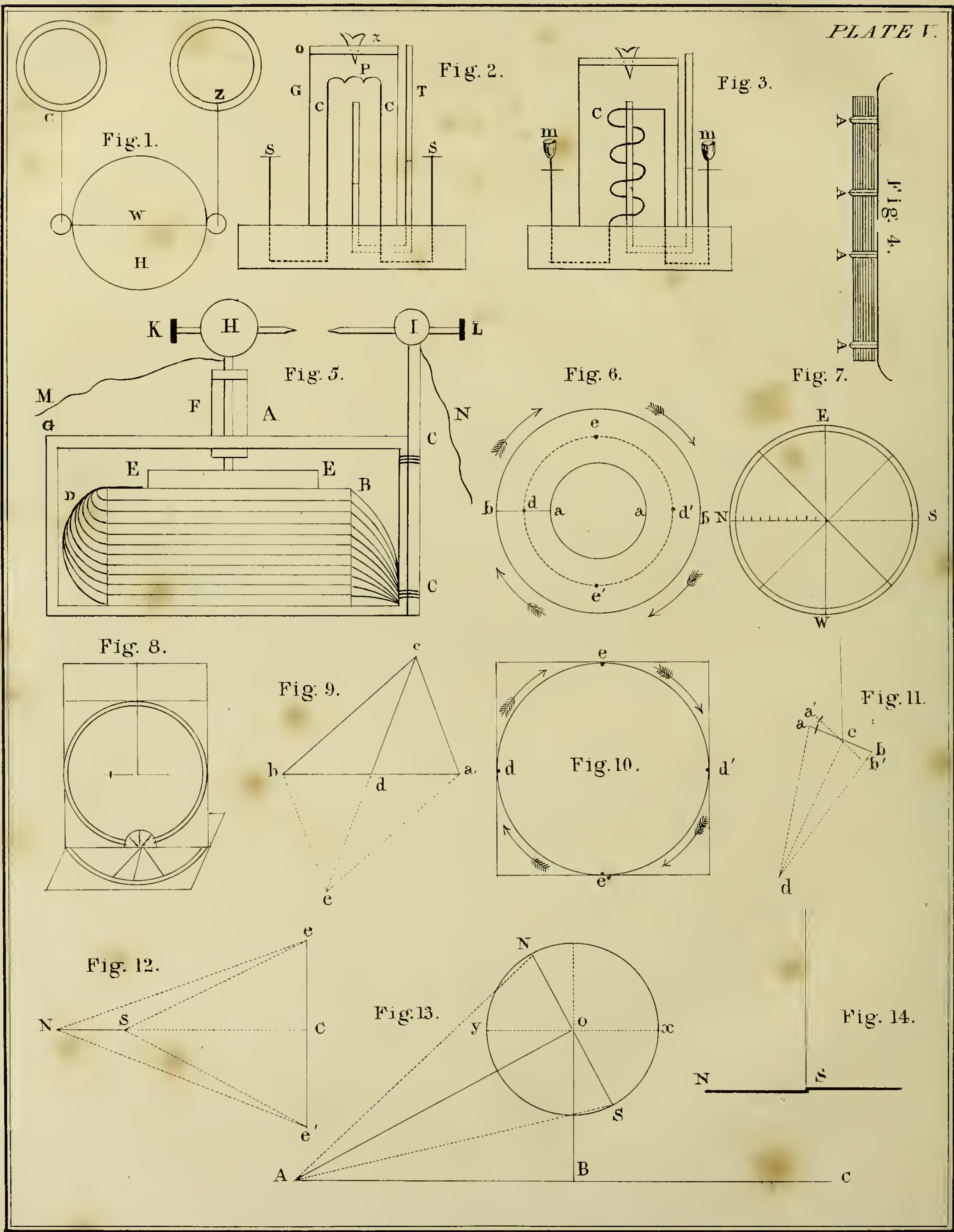
87. The *time* required to bring the mercurial column of the gage down to a quarter of an inch, is very great when compared with that necessary to reduce it only to one inch; even when the best pumps are used. In the experiments I have described, the *time* of pumping was particularly attended to; the standard being *thirty seconds:* which, under those circumstances necessary to guard against the agitation of the apparatus, was the shortest period that the pump which I employed would allow to bring down the mercury to one inch. Under all these circumstances it is obvious that much caution is necessary whilst carrying on experiments of this delicate nature; and that the standard attenuation of air, which I have employed, is much better calculated to give exact results, than when the attenuation is carried on much farther. And as the propagation of novel facts is always facilitated by simplifying the means of producing them, I have been anxious to place these within the reach of every experimenter: hoping they will be the means of removing some of those theoretical prejudices which have so long rested on the *report* of one solitary experiment.

Westmoreland Cottage.
Nov. 26th, 1837.

ERRATA.

Page 25, line 10, for *place* read *space.*—Page 32, bottom line, for *elastic* read *electric.*

LONDON ELECTRICAL SOCIETY.



Alvey, Lith., London Road.

RESULTS OF AN EXPERIMENTAL SURVEY

OF AN

ELECTRO-MAGNETIC COIL.

BY

CHARLES V. WALKER, Esq. Mem. Elec. Soc.

FROM THE TRANSACTIONS OF THE LONDON ELECTRICAL SOCIETY.

LONDON:

PRINTED BY STEWART AND MURRAY, OLD BAILEY.

1840.

XVIII. *Results of an Experimental Survey of an Electro-Magnetic Coil.* By CHARLES V. WALKER, ESQ., *Mem. Elec. Soc.*

Read 5th May, 1840.

1. These enquiries were entered into during the latter portion of October last.

2. The coil employed, which is the property of Mr. Gassiot, contained 300 feet of copper ribbon $1\frac{1}{8}$ of an inch wide, insulated in calico. Its dimensions were,—diameter $20\frac{1}{2}$ inches,—diameter of hole in centre 8 inches, leaving, as it were, a *ring* of effective coil $6\frac{1}{4}$ inches in width. This ring contained 82 convolutions of the ribbon, averaging, therefore, 13 to the inch. Besides being *terminated* with caps for mercurial connections, it had others placed at intervals of 50, 80, 120, 200 and 250 feet from the end, by means of which any required portion of the coil could be used at pleasure.

3. The direction of the convolutions, beginning from the centre *outwards*, was as the hands of a watch, from left to right.

4. The coil was further furnished with a plan of the horizon, divided into points and quarter points, having its radii divided into inches. This plan is 18 inches diameter, giving, therefore, 9 effective inches on each radius. It was duly placed on the surface of the coil.

5. Figures 6 and 7, Plate V., represent the coil and horizon apart.

6. The needle prepared for this survey, was a common sewing needle 2·3 inches long; and was suspended as a dipping needle, by a fine thread, against a graduated circle, on a card placed perpendicular to the horizon. The base of this arrangement consisted of a circular card graduated, in case it should have been required for azimuth distances: the needle was suspended with its centre 3 inches above this base. Fig. 8 is a sketch of this apparatus.

7. The battery used was *one* cell of Daniel's, excepting in two or three *experimental* instances, of which notice will be taken hereafter.

8. The connecting cups were numbered 1 to 7 from the centre outwards: the centre cup was No. 1., that on the outer circumference No 7.

9. The first series of results were obtained by placing the frame, which supported the dipping needle, over the *centre* of the coil, or the *horizontal* plan (fig. 7,): and then connecting the positive end (the copper end) of the battery with cup 1, and the negative with cup 7, the dip was observed to be about 90°. The connections with the battery being still retained, the frame, with the needle, was removed along the radius of the horizon, northward to *one* inch from the centre of the coil, and the dip was again observed; it was then removed to 2 inches, 3 inches, &c. to 9 inches, the dip being each time observed. The angles are given in the first column of Table I.

10. In these instances it will be observed that the motion of the electricity through the coil was in the direction of the hands of a watch; and, therefore, as might be expected, the south end of the needle dipped; and this dip was towards the *centre* of the coil.

11. The only result of reversing the battery connexions was to produce PRECISELY the same amount of dip, and in the same direction; but with the *north* end of the needle.

12. I next surveyed the radius from the centre toward the north-west, and found that, at corresponding distances from the centre, the dip exactly coincided with the results obtained on the north radius; this will be seen by comparing columns 1 and 2 of Table I.

13. I then tested, at random, the different radii, and found that the dip was always toward the centre, and that its angle was the same at equal distances. This being decided, I determined to confine my experiments solely to the radius pointing north, and to send the current always in the direction of the hands of a watch, as shown by the arrows, in fig. 6. It will be understood then that throughout I continued this arrangement; and that in the tables, (unless as in two instances in Table III.) the angles represent the dip of the *south* end,—that end of the needle which *points toward* the south.

14. In Table I. the contents of columns 1 to 12 inclusive will be easily understood, by inspecting their several heads. They all represent the survey of the north radius, (except column 2 as before remarked, § 12,) but with various portions of the coil excited.

15. Column 1 was obtained when the whole coil was in use; the positive terminal wire being placed in the first cup, and the negative in the last. There were then in action 82 layers of ribbon, in a space of 6¼ inches, commencing, as already mentioned, in cup 1, four inches from the centre, and terminating at the outer circumference of the coil, whose radius is 10¼ inches.

16. It is obvious that, till the compass needle was moved, and had reached 4 inches from the centre of the coil, it was not *directly* over any portion of the coil itself; but over the *hole* left in its centre. The portions of the columns, enclosed in the braces, are to point the eye to those situations, in which the needle was directly above an excited portion of the coil. Whatever figures in any of the 12 columns are not thus enclosed, will indicate that when the angle they represent was obtained, the needle was either over the centre hole, or over a portion of the coil not included in the circuit. The small figures mark the distance of the cup from the centre in each case.

17. By inspecting the Table, it will be seen, that when the results were obtained, as registered in columns 1 to 7 inclusive, the *outer* circumference *b, b,* fig 6, formed one of the boundaries of the excited portion of the coil; the other or *inner* boundary being altered for each column. And in the columns 8 to 12 inclusive, the *inner* circumference *a, a,* fig. 6, remained fixed, as the inner boundary and the *outer* varied for each.

18. Before adverting to the disposition of the forces employed to produce these effects, I will offer a few remarks on the angles here recorded, especially on those enclosed in the braces. By a simple inspection of these, the eye at once detects a remarkable regularity in the rate of variation. With the exception of columns 1 and 2, the difference of angular deflection, for each inch, is 10°, or (allowing for error of manipulation,) sufficiently near to 10°, to allow that number to be selected. As, for instance, in column 8 the angles are 10°, 20°, 30°, 40°, 50° and 59°. In column 9, 18°, 28°, 38°, 48° and 55°. In column 3, 20°, 30°, 40° and 49°. No such regularity as this can be found in the other portion of the other columns.

19. I have been led from this *general* view of a certain degree of regularity, to form an opinion which may be *established* possibly by analytical investigation, combined with future experiments:

It is well known that each layer of ribbon in the coil acts by induction, as it is termed, upon the contiguous layers; and that it is reacted upon by each neighbouring layer, producing those great and conspicuous results, which are the peculiar features of a coil; and which so manifestly differ in

degree from the effects produced by the use of a like length of metal uncoiled. But whether *each* layer, (when the equilibrium or greatest amount is produced), be under the *same* degree of excitement; or whether the *smaller* circles be more *highly* excited than the larger, or vice versâ, are questions which I conceive have rarely been proposed, or if they have been proposed, they remain at present unanswered. I presume not to propose the possibility of solving this interesting problem, from a mere glancing at the regularity of angular deviation, as a needle moves over a series of these layers, because I am well aware that such a solution can be deduced only from a strict and cautious mathematical investigation; but, I suggest, as an individual opinion, that the features presented by the portions of Table I. enclosed in the braces, strongly tend to induce a belief, that each of the 82 layers (or that each of any portion of these 82 layers excited,) is equally excited with every other. To express this generally: — When the circuit through a coil is completed, each convolution separately considered, is capable of producing the same effect,—is equally excited with every other.

20. There is no possible arrangement in which this can meet with *direct experimental* test, yet, I imagine the all-fertile science of numbers, in the hand of an experienced mathematician, will not be found wanting in all that is necessary to determine to demonstration, the truth or fallacy of this opinion.

21. Assuming the opinion to be correct, we are not altogether deprived of the power of experimentally investigating the subject; although such an end must be obtained by a circuitous route. For we might find the position of a resultant force which should represent the amount of these elementary individual forces, and substitute this for them, and observe the effect. The place of the resultant force may be thus obtained.

22. Let the width of the coil $a\ b$, fig. 6, be represented by the line $a\ b$, fig. 9; and let c represent the centre of an indefinitely small needle: let a be the first layer of ribbon toward the centre, and b the first layer at the circumference. Then these two will act on the needle in the directions $a\ c$, $b\ c$; by completing the parallelogram their resultant will be in the direction $c\ d$; and the same may be shewn to be the resultant of every other two layers of the coil.

23. And point d may be easily demonstrated to be *equidistant* from a and b:

For in triangles $b\ c\ d$, $a\ e\ d$, side $b\ c$ is equal to side $a\ e$; and angles $b\ c\ d$, $c\ b\ d$, are equal to angles $d\ e\ a$, $d\ a\ e$, each to each; therefore the triangles are equal in all other respects. Consequently side $b\ d$ is equal to side $a\ d$.

24. So that if a current of electricity circulated in the dotted circle $d\ d$, equidistant from a and b fig. 6, it should produce the same effect on an indefinitely small needle as the whole excited coil.

25. Each element of this circle $d\ e$, $d'\ e'$, fig. 6, of moving electricity, exercises its *individual* power in producing the deviation of the needle. To reduce this indefinite number of powers within due limits, let $d\,e$, $d'\,e'$, fig. 10, represent the same circle, with the current moving in the direction of the arrows; this circular current may be analysed into one moving along the four sides of the square, in the same direction. And the amount of force emanating from each side of the square, may be considered as concentrated in the points $d\ d'$, $e\ e'$, severally,—which spots are marked by the same letters in fig. 6.

26. These four forces may be still more simply represented by *one* so situated as to produce the same effect as these conjointly:

For if $a\ b$, fig. 6, be the north radius of the coil and the needle be placed over it, the effect of any

one convolution of copper ribbon, or the result of any *one* force, will be to place the needle at right angles to the *direction* of that force, with the two ends of the needle *equidistant* from the line of *direction ;* as in the isosceles triangle fig. 11 ; where *a b* is the needle, *d* is a portion of an electric current flowing *from* the spectator, in a direction at right angles to the position of the needle, and equidistant from its two ends *a* and *b.*

27. This may represent the position of the needle's equilibrium were it operated upon by only *one* force as at *d* fig. 6, but there is another *counter*-force of *equal* power, but more *distant* at *d'*, or the opposite side of the circle ; the effect of this (*alone*) would be to *reverse* the position of the needle, though to a *different* angle on account of the different *distances.* Its effect, united with that of the other portion at *d,* is to *increase* the dip to some other angle as *a' b'* fig. 11. So that the *position* of the resulting force, deduced from these two, is somewhere exterior to the resultant circle *d d'* fig. 6.

28. The two portions of the current at *e e'* fig. 6, must not however be overlooked. But as their influence does not affect the *dip,* on account of the positions, the whole effective force of the excited coil may be conceived as concentrated in the *two* points *d* and *d'*, figs. 6 and 10.

29. Before describing the trigonometrical formula, by which, in each experiment, the places of *one* force (the resultant of these *two*) was formed, it will be better to *explain* the circumstances of the positions *e e'* with respect to a needle on the line *a b :*

The effect of the current at *e* figs. 6 and 10 in the upper half of the circle, flowing from *left to right,* as indicated by the arrows, will be to deflect the south end of the needle towards *e ;* that of the current at *e'*, on the *lower* side of the circle, on the *other* side of the needle will be to deflect the needle towards *e';* these two effects destroy each the other ; and, as far as *deflection* is concerned, produce no effect on the needle. Again, since these *counter*-actions are equally compensating along the *whole length* of the needle there will be no alteration in the *dip.* With regard to the *north* end of the needle, the powers are repulsive, and each in like manner compensates the other.

30. In fig. 12 let C be the centre of the coil, and *e e'* the points in question ; then the dotted lines *e* S, *e'* S are the two lines of *attraction,* and *e* N, *e'*N those of *repulsion.* The resultant of the *former* will be in the line *N C* in the direction N C ; that of the *latter* in the same line, but in a direction from C the centre, to N. If these two forces were *equal,* they would counterbalance each other, and the result would be *no motion.* But because the end *S,* where *attraction* prevails, is nearer to the source of action than the end *N,* where *repulsion* prevails, we may theoretically expect that the *former* should have the pre-eminence, and that the needle should make an *entire* movement from its position of suspension towards the *centre* of the circle. This is the result of calculation.

31. On referring to my notes I find, when the needle was suspended over 7 inches from centre, that, independently of the dip, the thread which supported the needle was drawn forward toward the centre of the coil $\frac{1}{8}$th of an inch, when *one* battery was used ; $\frac{2}{8}$ths when *two* were used, and $\frac{3}{8}$ths when *three* were used. These are results when the battery was in series, *intensity* being increased for each experiment, but *quantity* remaining the same. When *two* batteries were used, *not* in series, according to Mr. Mason's plan,* the motion towards the centre was $\frac{1}{8}$th in. precisely as before ; because the *intensity* or attractive force was the same, although the *quantity* of electricity was increased.

32. The distances of divergence here were chords of an arc, whose radius was the *thread* of

* Vide Trans. Elec. Soc. page 68. § 61.

suspension, fig. 8 (§ 6.), four or five inches long. These facts seem to furnish a means of measuring the ratio of increase, produced by altering the battery, which cannot be obtained by other means.

33. I will now refer to column 1, Table IV. This contains the respective positions, in which a single force should be placed in order to produce the effects of the *two* forces $d\ d'$ fig. 6, into which that of the *whole* coil has been analyzed. It is obtained thus:

In explaining fig. 11 (§ 26.), I have shewn that $d\ c$ is the direction of the resultant force, which produces equilibrium in a needle, viz., a line which bisects the vertical angle of an isosceles triangle, whose base is the needle.

In fig. 13, let $N\ S$ be the needle dipping, and angle $N\ o\ y$ equal to angle $S\ o\ x$, the angle of dip. The lines $N\ A$ and $S\ A$ form the sides of the isosceles triangle, with the needle for a base; and the line $O\ A$, which bisects the vertical angle, and therefore is perpendicular to the needle, is the *direction* of the force. A, therefore, will be the *place* of the force; and the line $A\ B$, its distance from the place of the needle. Add to this $B\ C$, distance from the centre of the coil, and the sum, $A\ C$ will give the distance of the resultant force from centre for any angle.

34. The only calculation here needed is the simple trigonometrical formula;

Sin. ∠ O A B : O B :: sin. ∠ A O B : A B. In this equation $O\ B$ is *constant*, being 3 inches, the height of the needle, fig. 8 (§ 6.) The angle $A\ O\ B$ is equal to the dip, angle $S\ o\ x$; and the angle $O\ A\ B$ is the *complement* of the dip.

To produce the first result in Table IV, the equation is $\frac{\text{Sin. } \angle\ 79^{*} \times 3}{\text{Sin. } \angle\ 11^{\dagger}}$ or log. sin. ∠ 79 + log. 3 — log. sin. ∠ 11 = log. A B = log. 1·188469 = 15·43 = A B.

Add to this 1 inch at which the needle was placed from the centre, and the result 16·43, as given in Table IV, Col. 1, is obtained. The angles used in these calculations are already given in Table I.‡

35. I may here, in a few words, advert to the columns 2 and 3 of this Table, which contain the positions of the *same resultant* obtained from the first and second columns of *Table* 2.

36. The same equation is used for this. But $O\ B$ is *not constant*, as it varies with the elevation.

37. The general conclusions which I deduce from these experiments, are, *that the electrical energy existing in each layer of the coil, is equal to that in any other layer, whether toward the centre or toward the circumference; and, that the chord of the arc of the thread's deviation from a perpendicular, is a measure of a condition in the coil not easily estimated by any other mode.*

38. The results given in the various tables, I submit to the Society as data very carefully obtained, and *correct* representations of the angles they profess to record.

39. The Tables of calculations, *Table* IV, are given as resulting from the combined efforts of theory and experiment. And they represent the places of a *single* force calculated to produce the *same* effect as the *whole* coil. Should these positions stand the test of Ampère's formula, then the opinion advanced in favour of the *equal* distribution of the power would be established beyond doubt.

40. There is a brief list of results recorded in Table III, to which I have not as yet adverted. I

* Vide, Column 1, Table I. † 90°—79°=11°.

‡ Mr. Walker submitted the trigonometrical calculations whereby the positions in Columns 1, 2 and 3, Tab. 4, were obtained. They are not printed, as they are only the above varied for different angles.—Ed.

have not attempted any deduction from these, other than the general appearance of regularity evidenced in those portions which are obtained while the needle was over the coil; which regularity again furnishes further presumption that *electricity in motion is* EQUALLY *distributed along the whole line or path of its transit,* unlike electricity of tension, which accumulates at the *surface.*

41. This Table is the result of experiments, when the arrangement of the needle was altered. The same needle was removed from its frame, and to its south end was attached a piece of thin copper wire, which increased the length *twofold;* thus lengthened it was suspended from its centre, this centre being the south end of the needle, as in fig. 14.

42. Since one end of the needle is the centre of motion, the only forces in action will be those affecting the north end; and thus it seems possible to obtain a *less complicated* equation in order to apply Ampère's formulæ. But as the present communication has already extended to an undue length: and as the mathematical investigations necessary to demonstrate the subject will require considerable amplification, I will close now, leaving, however, the data in the Tables open to the use of any who may feel inclined to pursue the subject.

CHARLES V. WALKER.

Kennington, May 3rd, 1840.

After the paper was read, Mr. Walker explained the mode of surveying the coil, and observing the angles, and illustrated his observations by repeating some of the experiments on the ribbon coil belonging to the Gallery of Practical Science.—ED.

TABLE I.

This table contains the angles of dip for the south end of a needle, suspended from its centre.

Place of the needle, at an elevation of 3 inches.	(1.) Positive terminal wire in cup 1, Neg. in cup 7.	(2.)	(3.) Pos. in cup 2. Neg. „ 7.	(4.) Pos. in cup 3. Neg. „ 7.	(5.) Pos. in cup 4. Neg. „ 7.	(6.) Pos. in cup 5. Neg. „ 7.	(7.) Pos. in cup 6. Neg. „ 7.	(8.) Pos. in cup 1. Neg. „ 6.	(9.) Pos. in cup 1. Neg. „ 5.	(10.) Pos. in cup 1. Neg. „ 4.	(11.) Pos. in cup 1. Neg. „ 3.	(12.) Pos. in cup 1. Neg. „ 2.
Centre.		N.W. radius.				74°	61°					
1 in. from cent.	79°	79°	78°	76°	75°	72	59	78°	75°	75°	70°	70°
2 in. „ „	73	73	74	71	70	70	58	71	70	69	65	61
3 in. „ „	68	68	68	68	67	66	56	64	63	60	57	52
4 in. „ „	4 in. 60	60	62	62	63	63	51	4 in. 59	55	50	46	43
5 in. „ „	53	53	57	57	58	60	50	50	48	41	37	30
6 in. „ „	45	45	5½ in. 49	49	51	54	50	40	38	30	25	5½ in. 22
7 in. „ „	38	38	40	6¼ in. 40	7 in. 42	50	45	30	28	20	6¼ in. 15	12
8 in. „ „	30	30	30	30	31	40	40	20	18	7 in. 10	8	8
9 in. „ „	20	20	20	20	22	8¾ in. (30	30	10	8¾ in. 8	3	0	0
							9¼ in.	9¼ in.				

TABLE II.

This Table contains the angles of dip for south end of needle, suspended from its centre.

Elevation of needle above the coil when the whole was excited.	(1.) Over centre of coil.	(2.) Over 2 Inches north of cent.	Over 4 Inches north of cent.
3 Inches.	80°		
6 „	72	65°	46°
9 „	68	58	38
12 „	55	45	30
15 „	45	38	22
18 „	35	27	16
21 „	28	20	11
24 „	22	15	9
27 „	16	10	6½
30 „	12	9	5
33 „	10	8	3½
36 „	9	7	3
39 „	8	5	2½
42 „	7	5	2
45 „	4	3	1
48 „	1	2	1

TABLE III

This Table contains the angles of dip for the lengthened needle, suspended at its south end.

Place of needle at 3 In. elevation.	Pos. in cup 1. Neg. „ 7.	Pos. in cup 3. Neg. „ 7.
Centre.	70° South Dip.	60° South Dip.
1 In. from cent.	59	57
2 „	53½	52
3 „	44	48
4 „	4 { 38	38½
5 „	28½	31
6 „	20	21½
7 „	10	6¼ { 12½
8 „	0	2
9 „	9 North.	7½ North.

Place of needle, elevated 3 Inches.	(1.) Position of resultant force from centre.
1 In. from cent.	16·43 In.
2 „	11·812
3 „	10·425
4 „	9·196
5 „	8·981
6 „	9·000
7 „	9·344
8 „	9·732
9 „	10·091

TABLE IV.

Elevation of needle above the coil when the whole was excited.	(2.) Position of resultant force. Needle over cent.	(3.) Needle over 2 In. from centre.
3 In.	13·52 In.	11·812 In.
6	18·46	14·87
9	22·27	16·4
12	17·13	14·
15	15·00	13·72
18	12·60	11·72
21	11·16	9·643
24	9·697	8·431
27	7·742	6·761
30	6·376	6·751
33	5·819	6·638
36	5·702	6·42
39	5·481	5·412
42	5·156	
45	3·146	
48	·8378	

XIII. *Continuation of Experiments, with a "Constant Voltaic Battery."—Third Communication.*

Read 5th February, 1839.

88. In the former accounts of experiments with a Constant Battery, which the Society did me the honour to publish in their Transactions, I hinted (§ 64.) the possibility, if proper precautions were taken, of establishing a law, that if the decomposing power of one battery be ascertained as x, and that of another battery as y, the power of the two combined, *not in series,* would be $x+y$. Or; that, if the individual powers of any number of distinct batteries be ascertained, the power of the whole, combined as described, would be the sum of these.

89. In order to explain more precisely the nature of the law alluded to, I would instance the experiments which were then described. (§ 54 & 64.) A battery of 20 cells released an inch of the gases in 37″. We had in action 8 batteries of the same average power; and it was inferred that, if these were so arranged as that the positive and negative terminals of each should meet in two distinct cups, to be connected with the respective electrodes of the voltameter, we should obtain 8 inches of the gases in the same time, or 1 inch in $4\frac{5}{8}''$. This, however, was not the case; for we were unable to decompose at a rate greater than to produce an inch in 7″. (§64.) To one circumstance, tending materially to prevent experiment, in that instance, from coinciding with theory, I would at present first allude,—the great extent of wire required, (from the peculiar position of the batteries,) to form the necessary connections. I felt the force of this in my after-reasoning on the subject, and could very readily admit it as one important cause operating against the full development of the electromotive power of the batteries.

90. Since that time a new battery has been constructed by Mr. Mason, consisting of 99 cells; which, with the voltaic elements contained in them, were in a slight degree larger than those formerly used at Clapham. The zincs also were all amalgamated.

91. The whole was divided into 9 batteries, of 11 cells each, and was so placed that the connections for the experiments, *not in series,* might be made with the least possible quantity of the wire; that so a considerable portion of one obstacle might be removed, and the results be more nearly the simple and undiminished powers of the batteries. The experiments were conducted at the house of Mr. Mason, our party consisting of the same individuals as before. (§ 86.)

92. We commenced charging the battery at 3 P.M., on Wednesday, December 26, 1838, and continued it in action till 3 A.M. of the following day.

93. I shall not proceed to detail the results of our various experiments in the precise order in which they occurred, but rather arrange them so as to shew their bearing on the special objects now in view. I mention objects, because, independently of the evidence connected with the comparative electrolytic powers, we were also enabled to obtain an interesting result connected with another power of the battery, an account of which will be given in the sequel. (§ 121., &c.)

VIII. Insulation.

94. On former occasions we used the batteries placed on slips of varnished glass (§ 11 & 53.): but, from many causes, the insulation was far from perfect. In the present instance, great care was taken

in placing the cells, that no fluid should be spilled on the table or varnished glass supporting them; and every other circumstance concurred in offering the fairest prospects of using an insulated battery. The following is an extract from the notes, taken at the time, of the mode employed to test the insulation.

95. "Half-past six, P.M.: the battery at this time was *in series*, forming a single battery of 99 cells. One end of a wire was placed in the mercury cup, attached to the positive cell of the battery; its other end being at the same time in connection with a very delicate galvanometer. Another wire, (also in connection with the galvanometer), had its extreme end placed on the surface of the table, which supported the battery; but no deflection was obtained. Hence, it is fair to infer that the whole was exceedingly well insulated: so well that the table formed no conducting path between the cells. This test was varied, by removing the end of the latter wire from the table, and placing it against the outer side of a dry cell: still no deflection. When moved against a *wet or damp* cell, the needle spun round with great energy. When placed on an apparently insulated drop of water, resting on a slip of the varnished glass, no deflection."

96. Having thus learned the condition of our apparatus, we felt greater confidence in the results of experiment being correct: because we were now fully assured that no power was lost by passing from cell to cell.

IX. Decomposition.

97. The electrolyte was a mixture, by measure, of 1 sulphuric acid, with 6 water: specific gravity 1·189. The Voltameter (No. 3.) has been already described. (§ 39.)

98. The following table gives the time occupied in obtaining a cubic inch of the gases, with each battery.

TABLE F.

Bat. 1.	Bat. 2.	Bat. 3.	Bat. 4.	Bat. 5.	Bat. 6.	Bat. 7.	Bat. 8.	Bat. 9.
70″	70″	107″	111″	99″	103″	104″	113″	97″

99. The sum of these—874, divided by 9 gives the mean of the whole $97\frac{1}{9}''$. This divided by 9 gives about $10\frac{7}{9}''$, as the time, according to the proposed law, for the release of an inch of gases, if the batteries were arranged *not* in series. That is to say: if the proposed law be correct, an inch of gases should have been released in $10\frac{7}{9}''$.

100. On putting this to the test, using the same Voltameter (No. 3.), we were disappointed in the result; for, in four distinct experiments, the time was nearly double, being 21″ instead of $10\frac{7}{9}''$; so that here, we were more distant from the object in anticipation than before. (§ 64.)

101. This, however, was not decisive: for though that Voltameter might admit freely the passage of a current, whose quantity was represented by the metallic surface in *one* cell of the battery, yet it by no means followed that it should be equally open to the passage of a current nine times as great in quantity. Indeed, a little reflection would lead to the suspicion that it *would* not, that it *could* not admit the whole compound current to pass.

102. That it did freely admit the *former* current was proved by a previous experiment, with the whole battery *in series;* when the time required to evolve an inch of the gases was 55″. When another Voltameter (No. 4.), with larger electrodes, (§ 43.) was substituted, the time remained the same, namely 55″; thus shewing that, having a Voltameter, (No. 3.) whose electrodes were of size to admit the passage of an intense current of a given quantity, no advantage was gained by substituting another Voltameter, (No. 4.) whose electrodes were more than sufficient to admit the passage of the same.

103. But, when this latter was used with the arrangement *not in series,* the advantage derived from its larger electrodes was obvious. They were found to present an exit and an ingress sufficiently large to allow the passage of the entire compounded current, and gave a result precisely consistent with that previously obtained from calculation. The calculated result as already mentioned, (§ 99.) was $10\frac{7}{9}$″. The obtained result of three consecutive experiments was 10″.

104. The glass in which the gases were collected, was then changed for one more capacious, in which five inches were collected in 52″, averaging an inch in $10\frac{2}{5}$. This result requires a slight correction for hydraulic pressure; because the height of water remaining in the receiver at the termination of the experiment, was not the same with that remaining after an inch of gases was collected in the previous three.

105. The evidence deduced from these is remarkably in accordance with the anticipations of theory. And this is still more confirmed from the fact, that wherever the experimental results differ from those of calculation, using the same Voltameter, their difference is one of defect, not of excess; they are *below,* not *above* the result assumed. That they should be deficient is naturally to be expected, from the many additional mercurial connections, and the increased length of wire through which the separate currents travel, ere they are collected into one; even if no *other* cause intervened to check the development of the full powers of the batteries.

106. At different times, in the course of the evening, the same investigations were made with the batteries, combined as in the following table; by examining which, it will be seen, that the general features presented by each experiment accord with the assumed law.

TABLE G.

	Voltameter 3. Elec. ·039 in.		Voltameter 4. Elec. 1·125 in.	
	A.	B.	C.	
Battery 1.	70″	Calculated Results.	65″	Experiment 1.
	—		63″	,, 2.
	—		63″	,, 3.
,, 1 & 2.	43″	35″	37″	,, 1.
	43″		35″	,, 2.
	—		35″	,, 3.
,, 1 to 3.	34″	$27\frac{4}{9}$″	24″	,, 1.
	34″		24″	,, 2.
	—		24″	,, 3.
,, 1 to 4.	27″	$22\frac{3}{8}$″	18″	,, 1.
	27″		$18\frac{1}{2}$	,, 2.
	—		$18\frac{1}{2}$	,, 3.
,, 1 to 5.	25″	$18\frac{7}{25}$″	$\frac{82}{5}=16\frac{2}{5}$	,, 1.
	25″		$\frac{82}{5}=16\frac{2}{5}$	,, 2.
	—		$\frac{80}{5}=16$	,, 3.

„ 1 to 6.	24″	$15\frac{5}{9}$	$\frac{71}{5}=14\frac{1}{5}$	„	1.
	24″		$\frac{72}{5}=14\frac{2}{5}$	„	2.
	—		$\frac{71}{5}=14\frac{1}{5}$	„	3.
„ 1 to 7.	24″	$13\frac{3}{9}$	$\frac{62}{5}=12\frac{2}{5}$	„	1.
	24″		„ $=12\frac{2}{5}$	„	2.
	—		„ $=12\frac{2}{5}$	„	3.
„ 1 to 8.	21″	$12\frac{7}{50}$	$\frac{54}{5}=10\frac{4}{5}$	„	1.
	21″		„ $=10\frac{4}{5}$	„	2.
	21″		„ $=10\frac{4}{5}$	„	3.
„ 1 to 9.	21″	$10\frac{4}{5}$	$\frac{50}{5}=10$	„	1.
	21″		„ $=10$	„	2.
	21″		„ $=10$	„	3.

107. I would, in passing, direct attention to the decomposing power of Bat. 1., with Voltameter 3.; 70″ obtained at $10\frac{1}{4}$ o'clock, being the same as that previously registered, (§ 98.) which was obtained nearly six hours earlier from the same battery, namely, at $4\frac{1}{2}$ P.M.

108. The comparative power of the batteries with the different Voltameters will be seen, by bare inspection, and will tend to confirm the observation advanced, with respect to the efficiency of particular electrodes under particular circumstances.

109. It will be observed, that the fewer the batteries in action, the more nearly do the powers correspond; and that, when the whole nine were in action, the larger electrodes released *twice* the quantity of gas obtained from the smaller.* The comparative size of the electrodes was as 1 to 29.

110. It is therefore evident that Voltameter 3, is not a fit standard for comparing the relative powers of the batteries. Unfortunately, we neglected to obtain the *individual* powers of each battery with the other instrument. So that, in deducing by calculation the anticipated powers of the combined batteries, (represented in the preceding table by the centre row of figures B.) I had no other data for the investigation, than those obtained with the smaller Voltameter.

111. The mode of obtaining column B, is as follows:—From Table F, is selected the individual power of each of those batteries, whose united power is sought; for instance, batteries 1 to 5, being respectively 70″, 70″, 107″, 111″ and 99″; the sum of this is divided by the number of batteries, (in this instance 5,) and again divided by the same number; the result $18\frac{7}{25}$″ is placed in the table.

112. By bare inspection of the table, these, the results of calculation, indicate that the experimental results in column A, obtained by using the small Voltameter, fall below the standard in some proportion to the number of currents constrained to pass through the same electrodes; while those in column C, hover closely in all cases about the standard. That the errors in column C are of *excess*, not of *defect*, that the experimental results *exceed* instead of *fall short of* the calculated, occurs from the fact of the calculated column B being obtained from the powers of the small Voltameter: for, by noticing the *one* experiment recorded, (Table G.) in which *both* instruments were used, viz. the decomposing power of battery 1, it is seen that Voltameter 4 released the standard quantity in 5″ less time than did Voltameter 3. So that, had Table F, (from which column B, in Table G, is formed,) been obtained, by using the instrument with larger electrodes, the results in columns B and C, would have been even more nearly allied than they are now.

113. Bearing in mind these circumstances, and the many collateral and adventitious ones ever

* For instance, the proportion between the results when one battery was used, is 65 : 70; when two batteries were used, 35 : 43; when four batteries were used, 18 : 27; when eight were used, 11 : 21.

occurring in experiments themselves, I would suggest, whether we are not justified from the Tables already given in the Transactions, from the interesting communication presented by Mr. Mackrell, and especially from the comparison of columns B & C, in the present Table, to conclude that the following law has been experimentally established.

114. " *The electrolytic or decomposing power of any number of batteries combined* NOT IN SERIES, *is the sum of the individual powers of the same.*"

115. Any deviation from this law originates in the nature of the connections, or in the deficiency of size in the electrodes; but not in the powers of the batteries themselves.

116. The practical utility of this law, independently of its theoretical importance, will be readily apparent; in that, if we have at command a large assortment of batteries, of all kinds and sizes, and are desirous to electrolyze any compound to the greatest possible extent, we have merely to arrange these apparently heterogeneous elements into the form described, (§ 61.) and we obtain the sum of the powers of each, without the weaker in the least degree deteriorating from the powers of the stronger.

117. Before leaving the subject of decomposition, I extract from the notes the following experiments, with solutions differing in strength.

Nine o'clock, p.m.

Time to produce an inch of gases, (using Voltameter 3.)

Original solution, 1 acid, 6 water, by measure.				Spec. grav.	1·189—21″.
Diluted to	1 acid,	11 water,	....		22″.
„	1 „	23 „	....		25″.
„	1 „	47 „	....	„ „	1·063—30″.
„	1 „	95 „	....	„ „	1·034—40″.

---

118. With respect to the temperature of cells, during the time the battery was excited, I have little to record, but that generally, the positive cell indicated a slight excess above the negative; and that the temperature of each rose a few degrees during the period.

119. The heating effects at the ends of the terminal wires were observed in the following manner: The wires were fixed by two vices; their ends were distant $\frac{1}{10}$th of an inch, and the electric flame was allowed to play between them for five minutes. The vice supporting the positive terminal became sensibly hotter than the other. In like manner, when platinum terminals were attached to the ends of these wires and similarly treated, the end of the positive became sensibly red. Iron wire was substituted, and the same occurred but more conspicuously. In no case was the negative raised to a red heat. These are instances of the positive wire being heated *in* the circuit. The experiment in which the terminal end, *out* of circuit, was heated, has been formerly noticed. (§ 48.)

120. On looking over the notes, I find nothing peculiar in the heating powers on platinum wire. When the battery was in series, the only wire effected was $\frac{1}{200}$th in. diameter, from 10 to 14 inches being visibly red. The other arrangement ignited 11 in. $\frac{1}{100}$th, and 13 in $\frac{1}{50}$th diameter.

121. On a previous occasion, when the batteries were congregated to the amount of 320 cells, I had occasion to throw the first battery out of action, and observed that its negative cell, (being the

negative end of a series of 320,) was *very hot;* on testing, it was found to be from 20 to 30 degrees higher in temperature than the neighbouring cells. When another cell formed the negative end of a series of 300, the same effect occurred. We were unable to detect any peculiarity of this kind with the present battery of 99, which had formed part of the 320.

122. With the advantage of that extended series, 320, endeavours were made to obtain a striking distance, but without success. It was, however, suggested, that want of good insulation might operate unfavourably against the attaining the desired end. I have already described (§ 96.) the faith we were enabled to place in the insulation of the present battery, giving us a very fair opportunity of obtaining the desired information on this interesting point. Our mode of proceeding was as follows:—Two copper terminals were fixed in the vices before alluded to, (§ 119.) and their ends being approximated, with but a very thin stratum of air between, communication was completed with the battery, but the electrical light was not obtained between the two.

123. This was more satisfactorily shewn, by using a small instrument, belonging to Mr. Gassiot, with the view of determining this point. A wooden slide, supporting two pillars of brass, through which passed micrometer screws, terminating in small brass balls, was supported in one of the vices. The two balls were so adjusted as to leave only sufficient space for the passage between them of a single hair. The connections were then completed with the battery; but no electricity passed. So that, we arrive experimentally at the following conclusion.

124. "*A constant battery, of* 99 *cells,* IN SERIES, *does not produce electricity of sufficient intensity to strike through the thinnest measureable stratum of air.*"

125. In concluding this communication, I would remark that, in these and the other accounts of experiments, submitted to the Electrical Society, my main object has been to record faithfully the several effects obtained. I had no theory to support, and therefore registered the experiments simply as they presented results. Indeed, so little thought was directed, during the course of experiment, to the tendency of these results, that I was in *all* considerably, and in *some* instances entirely ignorant of their relative connections till after the whole was concluded, and the notes examined; and thus I was precluded, (even had the disposition existed,) from forcing any experiment to bend to the inclinations. Indeed, the arguments now gone through were quite an after result to the experiments; and were solely suggested by the remarkable similarity in the various results. I would merely add, that the previous communications paved the way toward objects which have been, as I should suggest, attained by the experiments now described, viz. the furnishing this Society with certain truths, based on the sure foundation of experimental fact: and I cannot but look back with pleasure to the protracted labours of December the 26th, because they were so varied as to furnish many important data not obtained on other occasions; which data have given confidence in submitting as established truths, *the law concerning the relative decomposing powers of batteries,* (§ 114.) and *the fact of being unable to obtain the striking distance;* (§ 124.) the two main causes which induced me to trespass again on the time of the Electrical Society, and to direct their thoughts afresh, at so early a period, to a subject of late introduced, I trust, not *too* often, to the attention of our members,—the constant Voltaic Battery.

CHARLES V. WALKER.

Kennington, January 29th, 1839.

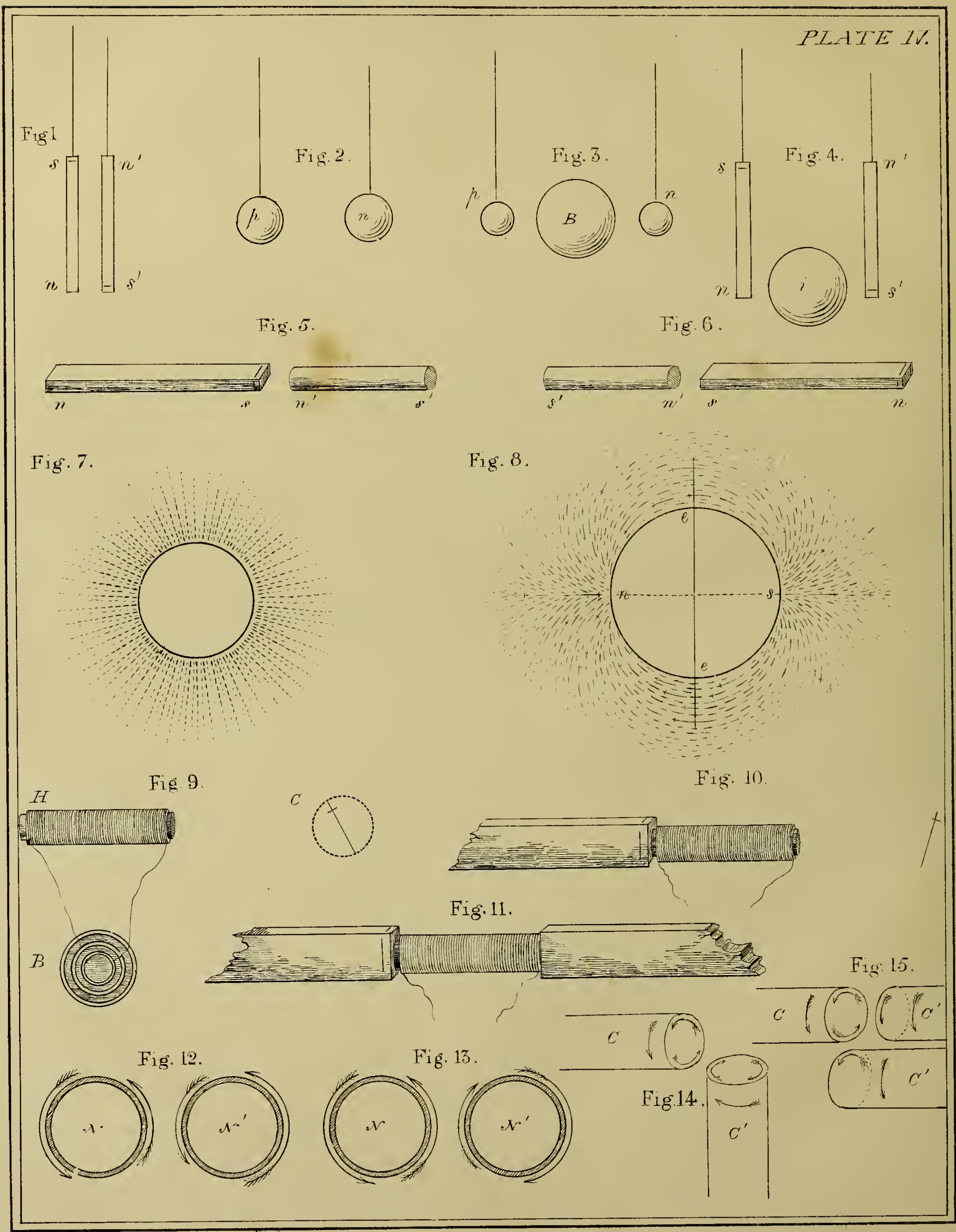


PLATE IV.
Fig 1
s
n'
n
s'
Fig. 2.
p
n
Fig. 3.
p
B
n
Fig. 4.
s
n'
n
i
s'
Fig. 5.
n
s
n'
s'
Fig. 6.
s'
n'
s
n
Fig. 7.
Fig. 8.
e
n
s
e
Fig. 9.
H
C
B
Fig. 10.
Fig. 11.
Fig. 12.
N
N'
Fig. 13.
N
N'
C
Fig. 14.
C'
Fig. 15.
C
C'
C'

X. *Account of a Series of Experiments made with a large Magneto-Electrical Machine.* By Mr. E. M. Clarke.

Read September 4th, 1838.

In presenting an account of the following series of experiments to the London Electrical Society, I do not hesitate to state those in which I was unsuccessful, as well as those in which I succeeded—although it may be evident that the former preponderates. At the same time, I may with truth affirm, that I consider the new facts elicited, more than compensate me for the time and labour employed in their development.

The Magneto-Electrical Machine, with which the experiments I am about to detail were performed—I believe, greatly exceeds, in dimensions, any hitherto made; its construction was commenced by me as far back as May 1837; it consists of 10 steel bars, each 4 feet long, but in the usual form of a horse-shoe, and the whole weighing 156 lbs.

My first arrangement of the machine was dividing the magnets into two equal parts, these were connected together by a common axis upon which rotated the inductors. The Quantity arrangement being at one side, the Intensity arrangement at the other. The results obtained with the machine in this form, were so opposite to what I had anticipated, that I suspected my arrangement was defective. The Quantity inductor was as usual furnished with a short coil of thick insulated copper wire, and the Intensity inductor with 7860 yards of fine copper wire. On trying the intensity arrangement with the voltameter, to my astonishment, no decomposition took place, although the shock obtained from it was most excruciating, I may add, even dangerous. Prompted rather by the spirit of experimental inquiry than encouraged by any expectation of obtaining such an effect as that which actually resulted, I next tried the decomposing power of the Quantity inductor, by which I obtained one cubic inch of the mixed gases in four minutes.

This being a novel fact, not only to me, but also to those scientific friends, to whom I mentioned the circumstance, I was induced to imagine, that the cause might be traced to a sort of compound action produced by the rotation of the two inductors, in the way I have already pointed out. I therefore determined to arrange the magnets in the usual manner, which I shall proceed to explain. The arrangement is similar in every respect to that of the machines which I have been in the habit of constructing, the only difference consisting in the size of the instrument, and the means of communicating motion to the inductors — which in this machine is effected by a crank and treadle, similar to the lathe.

The experiments are fully detailed in a table which accompanies this paper. It may, however, be as well first to give a summary account of the principal experiments; and in proceeding to do so, I feel it to be a duty to state, that I am indebted to a gentleman for his valuable suggestions, more particularly in reference to the substitution of wood for brass as a means of retaining the wires on the inductors. The gentleman to whom I allude, is Prof. Von Ettingshausen, of the University of Vienna. I have found the Professor's suggestions truly valuable. In this magnetic machine, I have made use of ivory for the purpose above mentioned; the brass plates, as the Professor suspected, giving rise to uncertain results owing to their conducting property.

I shall first proceed to notice,—the novel results of the experiments which I may briefly state to be,—first, the great amount of gases obtained in a given time by the Quantity inductor, a result which confirmed my opinion of the correctness of my original arrangement, although it must be confessed that my knowledge of the fact was very dearly gained, not only in a pecuniary point of view, — but also by the loss of a great amount of time and labour.

The second part to which I would request attention is, that I have never obtained any but a very trifling decomposing effect from the Intensity inductor, which will be at once perceived by an inspection of the table. It now becomes necessary that I should state, that the voltameter which was employed in the above experiments, was furnished with two slips of platina, an inch in length and three-eighths of an inch in breadth. It occurred to me, to vary the experiment by substituting fine pointed wires of platina, when the effects were materially altered, the decomposing power of the intensity inductor being then increased at least five times. These results appear to me to be singular, and as I am entirely an experimentalist, I shall not attempt to theorise upon the subject.

The next experiments to which I shall briefly allude, relate to the different nature, or I should perhaps rather say, the different appearance of the spark with various modifications of the inductors.

With the Intensity inductor a long, straggling, noiseless spark is obtained, having much resemblance to the spark which passes from the prime conductor of an Electrical Machine to a conducting body placed at about the limit of what is called the striking distance.

The Quantity inductor gives a spark which not only has the usual stellar form, but is accompanied with a loud snapping noise resembling the discharge of a Leyden jar. It may be as well to remark, that although these distinctions exist between the sparks, they both appear equally luminous.

I now beg to refer you to the table, which is arranged in such a form as to place at one view the results obtained by this arrangement of the Magneto-Electrical Machine.

Intensity Arrangements.

No.	Size of Inductor Cylinders.		Plates.	Diameter of Wire.	Insulated with	Connected in	Length of Copper Wire.	Time obtaining a cubic inch of Gas.		No Platina Wire heated.
	Diam.	Length.						Hours.	Min.	
11	$1\frac{3}{4}$	$2\frac{7}{16}$	Brass.	$\frac{1}{38}$	Cotton.	1 coil of	1412 yards.	No effect.		
12	do.	do.	do.	do.	do.	2 „	706 „	2	20	
13	do.	do.	Ivory.	$\frac{1}{23}$	Silk.	1 „	554 „	2	4	
14	do.	do.	do.	do.	do.	2 „	277 „		12	
15	do.	do.	do.	do.	do.	1 „	894 „	5*	0	
16	do.	do.	do	do.	do.	2 „	447 „			

Size of Platina plates used in the above decompositions, 1 inch by $\frac{3}{8}$.

Experiment 15, repeated with fine-pointed platina wires, 1 cubic inch of gas in 20 minutes.

The light from charcoal by comparison.

Best...15...13...14...11...12...3...2...1...Worst.

The shocks by comparison.

Strongest...15...13...11...16...14...12.........3...1...5...4...9...2...6...10...7...Weakest.

Intensity spark.

Long thin spark, with flame 12, $\frac{1}{8}$ inch long...11$\frac{3}{16}$...14$\frac{1}{4}$...13$\frac{3}{8}$...15$\frac{3}{8}$.

Quantity Arrangements.

No.	Size of Inductor Cylinders.		Plates.	Diameter of Wire.		Insulated with	Connected. in	Length of Copper Wire.	Time obtaining a cubic inch of Gas.	Length of Platina wire heated.
	Diameter.	Length.								
1	2	2$\frac{3}{16}$	Brass.	$\frac{1}{10}$		Cotton & India Rubber.	1 coil of	72 yards.	1 min.	1 in.
2	do.	do.	do.	do.		do.	2 ,,	36 each.	5 ,,	$\frac{3}{4}$,,
3	do.	do.	Ivory.	do.		White Silk.	1 ,,	100 yards.	9 ,,	$\frac{1}{4}$,,
4	do.	do.	do.	do.		do.	2 ,,	50 each.	1$\frac{3}{4}$,,	1 ,,
5	do.	do.	do.	do.		do.	1 ,,	64 yards.	1$\frac{1}{2}$,,	1$\frac{1}{8}$,,
6	do.	do.	do.	do.		do.	2 ,,	32 each.	No effect.	$\frac{1}{8}$,,
	Size of Ovals.			Copper Ribbon.						
	Greatest Diam.	Least Diam.	Length.	Thick.	Width.					
7	3$\frac{3}{8}$	2o	2$\frac{3}{16}$	$\frac{1}{52}$	1	Thick paper.	1 ,,	13$\frac{1}{2}$,,	No effect.	No eff.
8	do.	di.	do.	do.	do.	do.	2 ,,	6$\frac{3}{4}$,,	do.	do.
9	Cylin.	2 n.	do.	$\frac{1}{400}$	1	Thin paper.	1 ,,	48 ,,	2$\frac{1}{2}$ min.	$\frac{3}{4}$ in.
10	do.	do.	do.	do.	do.	do.	2 ,,	24 ,,	14 ,,	$\frac{1}{2}$,,

Quantity Spark.

Best...4...5...9...10...7...1...2...3...6...Worst.

Scintillations.

Best...5...4...1...2......3...6...Worst.

Note read 2nd October, 1838.

I have already stated in the preceding paper, that in experimenting with the large Magneto-Electrical Machine, I obtained the greatest evolution of gas, from points, when the Intensity inductor was employed, and from plates when the Quantity inductor was employed. On repeating these experiments at the Polytechnic Institution, in the presence of Sir George Cayley, I was induced to compare the effects of the inductors, when both points and plates were used in con-

nexion, that is to say, I connected a voltameter, fitted with points, to one fitted with plates, and placed them in the Magneto-Electrical Circuit, first with the Intensity inductor, and then with the Quantity inductor, and found that with the Intensity inductor, when the points and plates were in connexion, I had increased effects from both, the delivery of gas being still the greatest at the points; but with the Quantity inductor, the points and plates remaining in connexion, diminished effects were obtained from both; the diminution, however, being infinitely greater at the points.

These distinctions were so considerable as to be obvious to the eye, and Sir George Cayley expressed perfect satisfaction as to the results, but afterwards I thought it advisable to repeat the experiments, and note them more minutely. The results obtained are arranged in the following Table:—

TIME IN EVOLVING A CUBIC INCH OF THE MIXED GASES.			
Intensity Inductor.		Quantity Inductor.	
From points 16′ 40″	From plates 6h. 40′	From points 5′ 33″	From plates 1′ 42″
Connected.		Connected.	
From points 14′ 26″	From plates 3h. 20′	From points 14′ 26″	From plates 6h. 40′

The general result, therefore, is, that the points with the Intensity inductor, and the plates with the Quantity inductor, produce the greatest effects; the latter arrangement being the more efficient of the two, while points and plates are unconnected; but that when these are in connexion, the points are rendered more efficient with the Intensity inductor, and less efficient with the Quantity inductor, their influence being in fact so modified as to be equally efficient with either inductor: but the efficiency of the plates, while it is doubled with the Intensity inductor, is so reduced with the Quantity inductor, as to be only equal to their inefficiency with the Intensity inductor when out of connexion with the points.

E. M. C.

Lowther Arcade, London.

XI. *On the Application of Galvanism to the Blasting of Rocks, communicated in a Letter addressed to the Treasurer of the Electrical Society,* by MARTYN ROBERTS, ESQ., Corresponding Member of the Royal Geological Society of Cornwall, &c.

Read 20th November, 1838.

MY DEAR SIR,

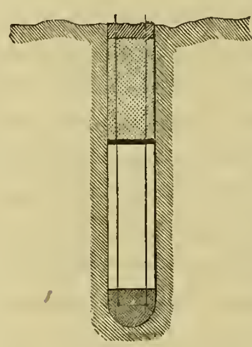
A recital of any experiments, tending to establish the use of Galvanism for practical purposes, I am confident will be received with pleasure by the Electrical Society, of which I have the honour of being a member; I therefore venture to send you a detail of my experiments, lately made, on the application of Galvanism to Blasting Rocks. They were performed on some granite rocks near this place; and the result was the most decided success, not only in establishing the certainty of firing the charge by the Galvanic fluid, but also the great increase of efficiency given to the charge of powder by this mode of firing, from a circumstance which has, I believe, hitherto been completely overlooked: to this I may add the total absence of danger to the workmen—a result which I am sure the Society will hail with pleasure, as many of its members must be acquainted with the deplorable accidents that have occurred, and are constantly occurring in the process of blasting: and if our Society has contributed, by its promotion of the Science of Electricity, to the saving of the life or limb of but one fellow-creature, I am certain all will rejoice in its existence.

The method of blasting at present in use amongst miners is this: — a hole of about twenty inches deep, and one inch and a quarter in diameter, is bored in the rock. In this is put about six inches of coarse gunpowder; over this is *rammed* a quantity of small stones, or pounded granite, until the hole is full. The gunpowder is fired by a *fusee,* (patented,) which has been in the first instance put in the hole leading from the month, through the small stones and gravel, (technically called "tamping,") down to the charge of gunpowder; the *fusee* being fired, burns a minute or two, according to its length, and ignites the charge at the bottom of the hole. The disadvantage attendant on this mode of blasting is, first, the danger in ramming the hole full of stones, or "tamping;" for, in tamping, it not unfrequently happens, that the metal rammer elicits a spark either from the tamping stuff, or from the rock itself: this spark ignites the charge, and the workman is either killed or maimed for life. — "Ramming the tamping" may therefore be considered a service of danger. The next disadvantage is the danger of the *fusee* hanging fire: this often happens, either from its bad construction, or from its being jammed in the tamping during the process of ramming. When it hangs fire for an unusual time, the workmen are induced to approach the rock, believing the *fusee* to be extinguished; and it not unfreqeuntly happens that, while doing so, the charge ignites, and the workmen are struck by the fragments of rock. I saw a poor fellow, last week, who was completely crippled for life by such an accident. It is also possible, (for it has occurred,) that the fusee, instead of hanging fire, may burn so quickly, as not to allow the workmen time to escape after having fired the end.—This, therefore, is another danger.

Again, a great inconvenience is the enormous loss of explosive force in the charge from the peculiar position and action of the "patent safety fuse," or indeed of all fuses whatsoever. The fuse is led from the mouth of the hole, through the tamping, to the gunpowder; and, in burning, its substance being destroyed, a vacancy or hole which was filled by the original substance of the fuse is thereby left; through this hole the charge of powder uselessly expends a great portion of its force,—the hole acting as a vent to the open air. We see a parallel case in cannon and fowling-pieces; for if we have too large a vent, or touch-hole, in a cannon, the shot is not thrown to half the distance it would be, if the vent were small; indeed, so great is this decrease of force, that a cannon, the touch-hole of which is even a little larger than its original size, is instantly condemned. How great, then, must be the loss to a charge of only one and a quarter inch in diameter, if a vent is given to it of a quarter

of an inch in diameter, — this being the usual size of the fuse. There is, therefore, a great loss of power from the burning away of the substance of the fuse. I have thus, I believe, shown that the inconveniences attendant on the old mode of blasting are various, and they may be classed under four heads.

1st, The danger of tamping. 2ndly, The danger of the fuse hanging fire. 3rdly, The danger of the fuse burning away too rapidly. 4thly, The loss of power from a vent being given to the charge. These inconveniences are entirely obviated by employing galvanism for igniting the charge, and by a method of tamping that I have introduced. To ignite the charge, I made use of a small battery of two pairs, each pair having a surface of forty-eight square inches. In the hole in the rock was placed two wires, joined at the bottom by a fine iron or steel wire; when connexion was made with the poles of the battery and these wires, the fine wire became red-hot and ignited the charge. Here then was no chance of either hanging fire, or burning too quick, for no ignition could take place, until connection was made with the battery; when made, the ignition was certain and instantaneous. The wires of communication in the hole, not altering their dimensions whilst conveying the igniting power, did not, of course, allow any vent through the tamping, by which the charge might expend its force. The increase this gave to the force of an ordinary charge of powder, was enormous, and well worthy the attention of miners, and all others connected with the operation of blasting rocks. The danger of tamping was avoided by a novel mode of filling the hole, which was found perfectly efficacious, as in no one instance did the tamping blow out. This is not perhaps a legitimate subject for discussion in our Society, but, as connected with the former part of my paper, may be allowed a hearing. It struck me that the expansive force of air might be applied with advantage in blasting. Knowing that air expanded rapidly with a small increase of temperature, I judged that if a body of flame could be thrown on air, it would expand with enormous force; this I tried, and with the most triumphant success. The experiment was as follows:—A hole of twenty-four inches was bored in a granite rock (see diagram); in this was put three inches of gunpowder; an empty space, or at least filled only with atmospheric air of twelve inches, was then left; then a wad of oakum was driven in nine inches from the top; over this was poured dry sand, and on this a small plug of wood: through the hole down to the powder passed the galvanic wires. The communication being made with the battery, the charge ignited, and the effect was prodigious. The bystanders, and they amounted to near a hundred miners, acknowledged it surpassed what they would have expected from double the quantity of powder, viz., six inches. Indeed, so minute did the quantity of powder I used appear to them, that before I fired the charge, they laughed at my attempt, and offered to sit on the rock during the explosion; but after the explosion, they changed their minds, and allowed it to be a wonderful improvement. This great increase of effect I attribute to the expansion of the air by the heat of the inflamed gunpowder, and also the mode of firing by galvanism, which allowed no vent for the charge to expend itself uselessly.



Hoping these facts will be found of interest,

I remain, my dear Sir,

Yours very sincerely,

MARTYN ROBERTS.

Rose Hill, near Penzance, Cornwall,
11th October, 1838.

XII. *Experimental and Theoretical Researches in Electricity. Second Memoir.* By WILLIAM STURGEON, ESQ., Lecturer on Experimental Philosophy at the Hon. East India Company's Military Seminary, Addiscombe, &c.

Read March 3rd, and December 19th, 1838.

On the Identity or Non-identity of Electricity and Magnetism—Different opinions of Philosophers on this topic—Experimental Examination of those Phænomena which are supposed to favour the hypothesis—Examination of M. Ampere's Hypothesis—The polar forces of hard steel Magnets unvanquishable by Electric-currents—The inefficiency of Electric-currents in magnetizing hard steel to a high degree of power—The distribution of magnetic force exhibited by Steel Magnets and by Loadstone, not imitable by Electric-currents.

88. In the first memoir which I had the honour to present to this Society, I endeavoured to elucidate those fundamental principles of electricity, which appear obviously developed by an extensive series of illustrative phenomena, and well calculated to afford an easy explanation of the nature and peculiarity of electric action. There still, however, remains one very important theoretical point on which I have not yet touched; a point which is yet wavering under the dominion of vacillating opinion, without any party venturing a demonstration of his peculiar ideas: or, indeed, showing much, if any, reason for entertaining them.

89. The discovery of the identity of lightning and ordinary electric discharges, by Franklin, and the well established facts of lightning depolarizing compass needles, reversing the polarity of others, and producing other remarkable magnetic phenomena, were events that have, long ago, led philosophers to imagine that electricity and magnetism are not distinct powers of nature: but that, more probably, they emanate, in different forms, from one and the same physical cause. The apparent similarity of the attractions and repulsions in magnetism and electricity, has also been considered as favourable to the hypothesis.

90. It is now more than half a century ago since the celebrated Father Beccaria ventured an opinion, that the electrical and magnetic powers are identical. "Are not these peculiar effects of the electric fire with respect to magnetism," said this eminent philosopher, "so many proofs which corroborate my former conjectures, that the peculiar magnetic force observed in *loadstone* is to be attributed to either atmospherical or subterraneous strokes of lightning: and that the *universal systematic* properties of magnetic bodies are produced by an universal systematic circulation of the electric element?"* This hypothesis of the illustrious Italian was not much attended to, till the discovery of electro-magnetism, which happened nearly fifty years afterwards; when it was again broached, as a new idea, by M. Ampere. Since that time the hypothesis has gained many proselytes, though there be still some philosophers who do not entertain that opinion: and as electricity has latterly produced many phenomena, whose true cause can only be understood by a proper solution of the problem which this disputed point has created, a strict investigation of the various circumstances connected with it can hardly fail to be interesting to the Electrical Society: I have therefore devoted the whole of this memoir to that particularly important subject, in which, it will be found, I have collected, examined, and arranged the most striking instances of analogy in electricity and magnetism: and have also pointed out many phenomena in which they as obviously disagree. I have contemplated the whole as profoundly as I have been able, and have discussed the various topics as I have proceeded, with freedom and candour, in the manner following:—

* Treatise on Artificial Electricity. By Father Giambatista Beccaria, p. 310, English edition, London, 1776.

91. If one of the poles of each of two magnets be presented to each other, a tendency either to recede from, or approach each other is immediately manifested, accordingly as these poles are similar or dissimilar respectively; and because similar and dissimilar electrized bodies evince corresponding tendencies to move *from* or *towards* each other, the two sets of phenomena have been regarded as marking a strong analogy, and have been held forth as evidence in favour of the identity of the magnetic and electric agents. But, before these, or any other supposed analogies be permitted to enter into any code of physical laws, they ought to be examined with the most rigid scrutiny and exactness. The phenomena ought not only to be compared with each other, but each individual event should be traced, as closely as circumstances will permit, to the nearest cause of its production; and in what manner it would be affected by varying the conditions of the experiment: and, in the question before us, it is only from such close investigations as these, that data are to be obtained which can be esteemed of much intrinsic value.

92. In contemplating the phenomena I have been speaking of in the manner proposed, let it be supposed that *ns s'n'*, fig. 1, Plate IV., are two magnetic needles, each suspended by a fine thread; and that *p* and *n*, fig. 2, are two dissimilarly electrized balls, suspended in a like manner. Then, because of the magnetic poles *ns' n's*, which are opposite to each other, being of different kinds, they will approach each other until they come into contact: and a parallel phenomenon will be exhibited by the dissimilarly electrized balls, *p*, *n*. Thus far the analogy appears to hold good. Our conclusions, however, are not to be drawn from these facts alone, for the motions already performed are the mere preliminaries to the display of other phenomena which demand still greater attention, and reveal the operation of other attributes than those which brought the bodies together. The electric balls, *p*, *n*, very shortly after the first contact, separate from each other; and if their first electric conditions were of equal degrees *above* and *below* the common standard, or neutral state, they would *neutralize* each other's action, and their fibres of suspension would hang parallel to each other. But if their first electric conditions were not of equal degrees above and below the natural standard, both balls would remain either *positively* or *negatively* electrical, accordingly as *p* or *n* exhibited the greater degree of electric tension prior to the first contact. In either case the balls would display a tendency to recede from each other, and diverge their fibres of support.

93. Now the motions last exhibited by the electric balls find no parallel phenomena in the magnetic poles *ns' ns'*, fig. 1, which still cling together without evincing the least tendency to separate: instead of which, it is a well-known fact, that the longer those poles are permitted to remain unmolested the greater degree of force would be required to separate them. Hence, then, without entering into any theoretical disquisition, these electric and magnetic phenomena are so obviously dissimilar, that instead of being susceptible of inferences in favour of an identity in the operating causes, they have an obvious tendency to bias the mind to the very opposite conclusion.

94. Let the two electric balls, *p*, *n*, fig. 3, be suspended on the opposite sides of a fixed ball B, which is in the natural electric condition. The electric bodies *p* and *n* will immediately approach B; and after contact with that body they will recede from it. When the body B is insulated, and the bodies *p* and *n* differ in degree of electric tension, *above* and *below* the natural standard respectively, all the three bodies remain electrized after contact: and *p* and *n* exhibit a tendency to recede from B. If, on the other hand, *p* and *n* are of equal degrees of electric tension *above* and *below* the natural standard, they will neutralize each other through the medium of B; and B also will remain neutral. If the body B were uninsulated, it would be a matter of no consequence in what manner *p* and *n* were

electrized, they would both become neutralized by contact with that body. Here then we have three conditions under which the electric balls, *p* and *n*, would approach B by electric action; but in no case would they be retained in contact with that body. In every variation of these experiments the bodies, *p* and *n*, would have their electric energies considerably deteriorated by contact with B; and in some cases those energies would totally vanish by such contact, however powerfully they might previously have been displayed.

95. Let now a parallel experiment be made in magnetics, by suspending two light bar-magnets by threads as represented by fig. 4. When the inferior dissimilar poles *n*, *s'*, hang on the opposite sides of a soft iron ball *i*, as in the figure, they immediately approach that ball; and when they have once come into contact with it they remain attached to it; and the longer they are left undisturbed the greater is their tendency to remain there: so that the contact, instead of diminishing the attractive force, absolutely increases it. How very different are these events to those which occur by electric action. In every case of contact by magnetic attraction, the forces which bring the bodies together, become exalted in some proportion to the closeness of contact: and in no case are those forces impaired by *time*. The electric attractive forces, on the contrary, are invariably, and immediately impaired by the bodies touching one another. In some cases they are suddenly and totally neutralized; and in no instance are they of long duration independently of a continuous exciting process.

96. Electro-polarization (52,) has an apparent analogy in magnetism, but the different ways in which the experiments may be varied, lead to results which show an obvious difference in the causes producing them. The nearest responsive fact is the polarization of soft iron by placing it in the vicinity of a permanent magnetic pole. If, for instance, the piece of soft iron *s'*, *n'*, fig. 5, be placed near to the magnetic pole *s*, of the steel bar *s*, *n*, a magnetic polarity will immediately be displayed in the iron bar: and arranged as indicated by the letters, viz. the south pole *s* of the magnet *n*, *s*, will cause a north pole in the vicinal extremity *n'*, and a south pole in the remote extremity *s'* of the iron bar: but if the north pole of the magnet be presented to the soft iron as represented by fig. 6, the order of polarity in the iron will be the reverse of that in the former instance: though still in accordance with the same law: for in both cases the poles in the permanent magnet occasion poles of the opposite kind to be exhibited in the nearest extremity of the iron: and polarity of the *same* kind in the remote extremities of the iron.

97. The circumstances under which the magnetic polarity thus displayed by pieces of soft iron bears so strong a resemblance to those necessary to the production of electro-polarity, (52, figs. 3 and 4, Pl. II.) that a superficial observer might easily be led to imagine that the same agency was in operation in both cases: but here, as in the cases already described, (92, 93, 94, 95.) a close investigation of these phenomena, and a correct view of those which a variation of the circumstances productive of them exhibit, lead to very different inferences. Let us, for instance, permit the pieces of soft iron, as in figures 5 and 6, to touch the permanent magnetic poles to which they are presented. The steel and iron would remain as decidedly polar as before: and the remote poles *s'* and *n'* of the two pieces of iron, and *n* and *s* of the steel bars would display still stronger polar forces than prior to the contact. These facts have no parallel in electricity: for if the electric bodies P and N, figs. 3 and 4, Pl. II., be brought into contact with the bodies *n*, *p*, and *p*, *n*, to which they are respectively presented, the phenomena of polarity cease to be exhibited: each pair of bodies immediately becomes similarly electric throughout; the one pair, fig. 3, being all in an electro-positive condition, and the pair, fig. 4, being in an electro-negative condition, on every part of their surfaces.

98. The electric phenomena displayed by bringing the bodies P, and *n, p*, fig. 3; and N, and *p, n*, fig. 4, Plate II. are easily explained by supposing an introgression of fluid *from* the relatively positive to the relatively negative bodies of each pair: but it would be exceedingly difficult to understand how the magnetic bodies maintained their polarity by any *similar* distribution of a fluid, or of any other physical agent, for whatever may be the nature of the magnetic agent, it is obviously more determinedly fixed or accumulated in the extremities of ferruginous bars by close contact, than when those bodies are at an appreciable distance from one another. Hence we discover that the magnetic and electric forces, which, at certain distances, effect such a similarity of phenomena in bodies situated in their respective localities, are productive of no corresponding facts when the approximation of those bodies is sufficiently close. Neither do the phenomena agree which the newly magnetized and electrized bodies exhibit after they have quitted those original magnetic and electric bodies whereon the respective disturbing forces reside; for, after the separation of *n, p*, and *p, n*, figs. 3 and 4, Plate II. from P and N respectively, the former would exhibit *positive* and the latter *negative* electric action: but the pieces of iron, figs. 5 and 6, Plate IV. would lose all traces of magnetic action, when once they were sufficiently removed from the localities of the magnets to which they had been attached.

99. If it can be imagined that by substituting steel for the pieces of soft iron in figs. 5 and 6, Plate IV. an analogy to the phenomena exhibited by the electrized bodies would have been more apparent, by the steel retaining magnetic action after quitting the disturbing magnetic poles, I would observe that its retaining some trace of magnetic action is a fact which cannot be denied: but in that case the steel would remain polar, as is always the case with magnetic bodies: and as no trace of polarity would be exhibited by the electric bodies, but on the contrary, an uniformity of electric action would be discoverable over every part of their respective surfaces, the *supposed* analogy again loses its support, and as decidedly fails in this instance as in those previously discussed. Moreover, the pieces of steel would retain their polarity unimpaired, even after long continued contact with other bodies; whereas the electric bodies would lose all trace of electric action by the slightest touch with uninsulated conductors.

100. A globe of steel may be made to exhibit *permanent* magnetic polarity when far removed from every disturbing force: but the same globe will not maintain any corresponding electric action. A plate of glass will exhibit electro-polarity, on its opposite surfaces, for some considerable time after it has been removed from the exciting apparatus: but magnetic polarity is not known to be exhibited by glass. If then the magnetic and electric elements be identical, why this capricious selection of bodies for the display of these parallel phenomena? The electric forces will attract all kinds of matter without exception; but the magnetic forces appear to be exceedingly select in this particular; operating on particular kinds only. Coated glass, whatever may be its form, affords no *permanent* electric attractions, which are, in the least, comparable with the attractions exhibited by magnetic bodies: for if a metallic arc connect the two sides of a Leyden jar, the electric forces immediately disappear; but an iron arc connecting the poles of a horse-shoe magnet is permanently held there, unless removed by mechanical violence; and the longer it remains undisturbed by extrinsic force, the more vigorously is it attracted by the poles; and there is no known substance whatever, by which the poles of a magnet may be connected, that will, in the least, deteriorate their powers.

101. Those few kinds of elementary matter on which magnetic attractions are known to be exerted, display no distinction of respect for the *north* or *south* polar forces, being attracted indiscriminately, and to the same extent, by both. Very different indeed are the nice discriminations of

the *positive* and *negative* electric forces manifested in an almost endless variety of phenomena, every one of which teems with interest in the contemplations of the philosopher, and beautifully characterizes the agency of their production. If, for instance, an intimate mixture of sulphur and red lead be indiscriminately projected through the air to a series of *positively* and *negatively* electrized surfaces, the powders will be separated from each other by the dissimilar electric forces, into whose spheres of action they are thrown; and the sulphur and red lead will respectively be found at the positive and negative surfaces, exhibiting a peculiarity of arrangement not known to be accomplished by any other kind of physical agency.* Similar selections are uniformly exhibited by electric forces, whenever the particles of compounds on which they operate are sufficiently voluble to be put into motion by them, or are held together by inferior powers. Every individual electro-chemical decomposition appears to be an instance of this kind of action, and demonstrates the peculiarity of this important fact.

102. It has been said by M. Œrsted, that the only difference in the electric and magnetic forces rests in their different degrees of tension or activity; the electric being the more active or vigorous in its operations: and this hypothesis has been attempted to be supported by M. Ampere and other philosophers, whose opinions on this subject will long command respect. But I must confess that I can discern no satisfactory discrimination of this kind, nor am I acquainted with any facts that are even in the least favourable to it. It is well known that electric attractions are the most powerful when the bodies exhibiting them manifest the greatest degree of tension in the display of all other electric phenomena. The spark, for instance, is shown to the best advantage when the electric body, whence it proceed, exhibits the greatest degree of attraction: and the charge of a jar is accomplished in the shortest period of time, and with the greatest degree of facility, under similar circumstances. Moreover, when electric discharges are performed, either from a single jar, or from a battery of jars, the striking distance is greatest, the flash is the most brilliant, the noise is the loudest, the physiological effects are the most powerful, and, in fact, every phenomenon is exhibited under the most advantageous circumstances, and in the most perfect manner, when the jar, or battery, is in the most suitable condition for a display of its attractive energies.

103. But now let us enquire into the *extent* to which electric attractions are usually exhibited. Has any electrician ever seen a prime conductor, (which always shows attraction more powerfully than any other electric apparatus) support, by its electric energies alone, a single *ounce* of any kind of matter? I presume not. If, then, with this insignificant attracting force, electricity be prepared for a display of some of its most splendid and terrific phenomena—the production of vivid light, intense heat, the noise of thunder, and the destruction of animal life: and that magnetism proceeds from the same cause or agency, it seems natural to ask, why it is that similar phenomena are not exhibited to the same, or even a greater extent, by a magnetised body whose attractions are ten thousand times ten thousand greater than any ever witnessed in electricity? These important questions, which stand so prominently and essentially in the path of investigation, demand the most profound contemplation of the philosopher, and must not be passed over in silence by those who are endeavouring to identify the electric and magnetic powers. We have yet to learn the mode of producing a *magnetic spark*, and are totally ignorant of the sensation communicated by a *magnetic shock*.

* This fact was first shown by Leightenberg. Cavallo and Bennet, especially the latter philosopher, have extended the original experiments of Leightenberg, and varied them in a variety of pleasing and interesting ways.—*Bennet's New Experiments on Electricity. Derby*, 1789.

And *magnetic chemistry* is so profoundly obscured from our knowledge, that no one knows even of its existence.

104. If our reasoning be permitted to rest on facts alone, independently of favourite notions and ingenious hypothesis, which are but too apt to captivate the imagination of the superficial observer, and, sometimes, even to sap the understanding of the more studious in science, the obvions contrasts in the phenomena presented by electricity and magnetism enforce themselves upon our notice too powerfully to be misunderstood. Even the attractions, themselves, in which *alone* the appearance of analogy exists, are so exceedingly dissimilar, so truly distinct from one another, that their peculiar characteristics are well defined and easily discernible, and cannot be mistaken by those who devote to them a proper and sufficient degree of attention.

105. An insulated electrized globular body *radiates* its attracting influence on every side alike, when surrounded by an uniform medium, such as the atmospheric air, as may be understood by fig. 7., which may represent a great circle of the globe with its radiating electric force. But a magnetized globe, similarly situated in space, exhibits no such radial influence; for being polar on opposite points (*n. s.* fig. 8.) of its surface, the greatest *disposable** attracting forces are exerted about those polar regions, and especially in the line of their axis continued. At right angles to that axis, in the plane of the equator, *e e*, the polar forces, by their mutual attractions, nearly balance one another; neither of them exhibiting much *disposable* influence on exterior bodies. Another great characteristic distinction in the display of the electric and magnetic forces by these bodies appears

* It appears by the distribution of iron-filings, when strewed on paper, above a bar magnet, that a considerable portion of the *north* and *south* forces are engaged in attracting one another, as shown by the curve lines assumed by the filings; and, consequently, are not employed, or, at least, very sparingly so, in any attractions which the magnet exercises on foreign bodies, such as pieces of soft iron, magnetic needles, &c., placed a few inches distant from its extremities and in a line with its axis; or, indeed, opposite to any other part of its surface; and, although much more of the magnetic force is brought into play as the iron is brought nearer, and most of all when it is in contact with the pole of the magnet, there is still a considerable portion of force which cannot be exerted on this foreign body, because of its being engaged with the opposite force, about the surface of the steel, which lies between its extremities; and especially that which is situated near to its centre. For convenience then, I call that portion of the magnetic force which lies about the equatorial part, the *engaged force;* and that which is brought into play on foreign bodies the *disposable force.*

The *disposable force* of any magnet may be diverted from its original directions of action by the approximation of ferruginous bodies; and, in some instances, nearly the whole of it may be drawn from a body on which it operates, without moving either the magnet or the body. To illustrate this point, let a bar magnet be placed six or eight inches distant from the pivot of the needle, and at right angles to its direction. The *disposable* force of the magnet will deflect the needle to some considerable number of degrees. Now place on each side of the magnet, parallel to it, and about three inches distant from it, a piece of soft iron, about its own shape and size. The deflection of the needle will lessen considerably, showing that a portion of the *disposable* force has been diverted from its action on the needle. Now, bring the pieces of iron nearer to the magnet, and the deflection again decreases: and when the pieces of iron are brought into close contact with the magnet, one on each side, from end to end, nearly the whole of the *disposable* force will be exerted on the iron, and but very little of it, if any, will reach the needle so as to cause a perceptible deflection. Now, in this case, the extremities of the magnet are still untouched by the iron, and are, consequently, as much exposed to the needle as when the iron was not present; notwithstanding which, it is obvious from the experiment, that the *disposable* force which before deflected the needle has now taken another direction, and is employed in polarizing the pieces of soft iron. The disposable force of the magnet, however, although it cannot now reach the needle with a sufficient degree of formidableness to accomplish deflection, is not entirely engaged by the iron, a residuum still remaining, which is detected by bringing the needle nearer to the magnet.

to be this;—the electric force is wholly *disposable,* and ready to be exerted upon, and even *transferred* to, other vicinal bodies: whereas the magnetic forces are neither *transferable* nor wholly *disposable,* for no magnet has yet been known to have its power impaired by contact with unmagnetized bodies, and in no case is the whole of its attracting power exerted upon a vicinal body.

106. I have been exceedingly anxious to discover, if possible, some facts which might afford analogies whereon to fix a basis of reasoning on the identity of these physical agents; but, although I have met with some further phenomena, far from being uninteresting in the discussion, a close examination of their true character has shewn their evidence in favour of the supposed identity to be of no more value than that afforded by the facts already noticed.

107. If there be one electric apparatus more than another, whose action resembles the action of the magnet, it is the dry *electric column,* whose polar forces are more uniformly and permanently exhibited than those of any other electrical instrument. But the attractive and repulsive powers of this instrument, like those in all other electrical arrangements, are exceedingly feeble when compared with the gigantic powers of a magnet; they are, moreover, directed towards, and operate upon, every kind of matter without distinction, whereas the magnetic attractions and repulsions, notwithstanding their vigorous action on ferruginous bodies, are, with the exception of one or two of the metals, perfectly inert on all other kinds of matter. The attractions and repulsions of the electric column are productive of vibratory motions in pendulous bodies properly situated between the poles; which show that the vibrating body changes its electric condition at every contact with either pole of the instrument, and accommodates itself to the attractive influence of the opposite pole. When the pendulous body has come into contact with the positive pole, it acquires an electro-positive condition, and is repelled to the negative pole, where it deposits its charge and becomes electro-negative. It is now again under the attractive influence of the positive pole, to which it is compelled to make another journey, and *from* which it receives a new charge and an immediate succeeding repellent impulse, which again directs it to the negative pole; and in this manner the suspended body performs its vibratory motions, being in an electro-positive condition whilst travelling in one direction, and in an electro-negative condition whilst travelling in the other. By these means a *pulsatory current* permeates the pile from the negative to the positive pole, the fluid being transported through the air, from the latter to the former by means of the pendulous body.*

108. Besides the pendulous motions already alluded to, the dry electic column is productive of physiological and chemical phenomena, will emit sparks and charge coated glass and other inferior conductors, as decidedly as charges are produced by the machine: all of which are so perfectly distinct from, so decidedly foreign to, any known capabilities of the magnet, that there is not to be found one solitary trace of analogy in the performance of the two kinds of apparatus. The attractions and repulsions are the only phenomena in which there is a *shadow* of resemblance, whilst in *reality* even this faint analogy has obviously no special existence. The delicate electric forces which alternate the conditions of, and give vibratory motions to the pendulous body, find no similarity of action in the majestic attractive forces of the magnet, which select those of their own species only; whose coeval polar affinities mutually exalt the action, and constrain the attracted body to assume a deter-

* As this discussion requires experimental facts rather than theoretical opinions, I have not, in this place, entered on the doctrine of the dry electric column. It is possible I may have occasion, at some other time, to enter fully into the philosophy of this interesting apparatus.

minate polar condition, and prevent its escape from the vigorous influence of the pole to which it is first attached. Hence as no vacillancy in the magnetic condition of the attracted body is produced, the grand essential to vibratory motion has no existence in magnetics: nor can any such locomotions, as those exhibited by the electric column, be produced by any known self-acting powers of the magnet.

109. If we are to look for the supposed identity of electricity and magnetism amongst electro-magnetic phenomena, we are still as far from arriving at satisfactory conclusions as in any other branch of the science. It is true, we here find some of the most striking and interesting affinities which electricity and magnetism have hitherto developed; affinities which will ever link these sciences together in the firmest bonds of physical union, though by no means identifying the elements by which the phenomena are produced. Each elemental agent plays its own part in the production of electro-magnetic phenomena as decidedly as in those of magnetic electricity, whose display is accomplished by the reciprocal excitement.

110. From the attractions and repulsions exhibited by wires carrying electric currents, M. Ampere was led to imagine that all magnets owe their influence to an unremitting circulation of the electric fluid; an hypothesis so exceedingly ingenious, and so eminently calculated to favour the expectations of some philosophers, that there can be no astonishment excited by its gaining proselytes amongst those whose minds were already predisposed for its reception. But, notwithstanding the respect which is due to the talents of those philosophers who have favoured Ampere's views on this topic, I must candidly confess that the hypothesis has always appeared to me to be much easier to acknowledge than to understand. In the present investigation I have considered experimental facts as the only data on which I can proceed with any chance of success of arriving at a close approximation to true theoretical inferences. I have, therefore, neither ventured an opinion of my own, nor permitted the views of others to influence the inquiry.

111. The imaginary electric currents to which Ampere refers all magnetic action, lead us to enquire into the character and situation of their source, and by what means they can be supposed to be *perpetually* and equably maintained, either on the surface, or within the body, of a steel bar. Here it is that we are led to enumerate and examine all the known artificial sources of electric excitement, and endeavour to trace their influence to the operations of permanent steel magnets. Independently of *magnetic* excitation, we know of only three sources of electric currents, viz. frictional, voltaic, and thermal: for besides these four, there are no other sources known:* hence, if a bar of steel which exhibits *permanent* magnetism has that power conferred upon it by the influence of electric currents, which must necessarily be as durable as the magnetic action itself, to which of these sources are we to look for the *supposed* actuating currents? Or are there other sources of electric currents of which we are yet entirely ignorant? But, from whatever source those imaginary currents may be supposed to proceed, that source must necessarily be situated either on the surface, or within the body, of the steel. The idea of electric currents being excited by *friction* amongst the particles of the solid metal, is too absurd to be entertained for a moment: and the conditions necessarily required for the production of *voltaic* currents, are no where to be found in the steel: hence our enquiries are necessarily limited to *thermal* excitation alone.

* The dry electric column is here omitted.

112. That thermo-electric currents are producible in every piece of metal, whether pure or compound, is a fact which I have proved by very extensive experiments, some years ago.* But it must be understood that to produce an electric current by any means whatever, requires a co-existent motion in some of the elements employed during the whole time the current is flowing: unless it be of a momentary duration only, and the effect of an impulse, in which case the current may continue to flow for a short time subsequently to the terminal exciting impulse. When a current is produced by an electric machine, the glass cylinder, or plate, as the case may be, is necessarily kept in motion. When a voltaic combination is the electric source, the *liberated* elements of the liquid in the battery are put into motion and become vehicles for the transportation of the electric fluid to and from the solid parts of the arrangement: and a thermo-electric current depends upon the motion of the calorific matter: for when that element is perfectly at rest in the combination, the electric current ceases to flow.

113. From the above considerations it appears, that a perpetual propagation of thermo-electric currents on the surface, or within the body, of a steel magnet would require a perpetual motion of caloric within its mass: which motion, unless the production of some hidden, mysterious and unsuspected agent within the steel, would require as continual an influx and efflux of the calorific element from and to the surrounding medium. Moreover, the laws of electro-magnetism require that the direction of the electric currents should be at right angles to the axis of the steel bar; and the ingenious author of the hypothesis has ventured to assert that their route is in that direction, in a series of parallel spirals round its surface.† Such, then, are the necessary conditions upon which Ampere's hypothesis essentially depends; and being now, probably for the first time, disrobed of their mysterious habiliments, I must necessarily resign the glory of their *discovery* to those philosophers who still entertain the idea of their existence in the steel, and who may possibly be enabled to penetrate the subject still deeper than I have investigated it. But before I quit this important topic, I will mention a few more facts, which to me, have appeared of some consequence, and can hardly fail to be interesting to others who may be induced to pursue the enquiry.

114. If the temperature of one extremity of a steel bar be elevated, and, by that process, electric currents become excited; those currents would necessarily be more powerful than any which can be supposed to exist in the metal at its natural temperature: and if the other extremity of the steel were to be heated, and again thermo-electric currents be produced in it, these latter currents would be propagated in the opposite direction to the former, and consequently the magnetic forces which they brought into play would be exerted in the reverse order to those which the first currents excited: and these artificially excited electro-magnetic forces being more powerful than any which the *supposed* natural electric currents could produce, they would predominate over these latter, and give new energies to the bar, reversing its poles in accordance with the directions of the currents. But on making the experiments, and carefully examining the phenomena, I find that no such corresponding changes have taken place in the polar forces of the magnet: and, although the poles themselves are considerably molested during the unequal temperature of the extremities and other parts of the magnet, and are removed from their original positions by the heating process, they do not assume those positions and variations of force which the thermo-electric current would necessarily give to

* Philosophical Magazine and Annals of Philosophy, vol. x. p. 1.

† Annales de Chimie et de Physique, t. xv.: and Ampere's Recueil des Observations Electrodynamiques.

them, were they governed by no other influence:* hence I infer, that thermo-electric currents do not constitute the sustaining power of the magnet.

115. I next subjected a steel bar magnet to the influence of electric currents proceeding from a voltaic pair of copper and zinc. The voltaic combination was of the cylindrical shape and size, which, as is well known, I have long employed for electro-magnetic purposes, the zinc being surrounded with brown paper or calico, to prevent contact with the inside of the copper; and the whole placed in a pint porcelain jar, the exciting liquid being a solution of nitrous acid in water. The magnet which I employed was of hard-cast steel;—cylindrical, and about 6 inches long, and $\frac{3}{4}$ of an inch in diameter. It was well polished on an emery wheel, and of considerable power. It would lift, by one of its poles, a piece of soft iron of its own weight. A piece of soft iron of precisely the same figure and dimensions as the magnet, was also provided. A single helix of copper wire, No. 13, of the same length as the magnet, was formed on a hollow pasteboard cylinder, of sufficient width for the easy introduction of the magnet or iron. With these preparations, and a compass-needle furnished with an agate cap, and supported by a fine steel point, the experiments were carried on in the following manner.

116. When the meridian line of the compass-box had been adjusted parallel to the needle at rest, the helix was placed on the eastern side of its pivot, with its axis in the same horizontal plane as, and at right angles to, the axis of the needle; the nearest extremity being 12 inches from the needle's pivot. Fig. 9, is a represention of the arrangement, where C is the compass-box, H the helix, and B the battery. Before the battery connexions were made with the helix, the magnet was introduced to the interior of the latter with its marked end nearest to the needle, consequently at 12 inches distant from its pivot. The south end of the needle was drawn towards the magnet a certain number of degrees, and this deflection being noted, the magnet was taken out of the helix, and replaced again with its poles in the reverse order, by which means the north end of the needle was drawn towards the magnet, which deflection was also noted. The magnet's action on the needle being thus ascertained, the electrical force of the battery was laid on, whilst the magnet was in the helix; and when the deflection arising from this combined force had been ascertained, the battery connexions were reversed, and consequently the directions of the current in the helix was reversed also. This last direction of the current gave a new deflection of the needle, which, after being ascertained, was also noted down. This done, the magnet was reversed in the helix; and when the deflections of the needle arising from the current traversing the helix in each direction respectively had been ascertained, the electric current was finally cut off, and the deflecting power of the magnet alone again ascertained in the same manner as at first.

117. The bar of soft iron was next placed in the helix, and the electric current again laid on; and when the deflection arising from the polar force of the iron, by the first direction of the current, had been ascertained, the battery connexions were reversed, and with them, of course, the polarity of the iron

* At the time this memoir was first drawn up, only a few experiments had been made on this part of the enquiry, the general results being such as are described in the text. But, whilst writing a fair copy for the press, I was led to reconsider this part of the subject, and it occurred to me, that by pursuing the experiments, some results might probably appear which would be interesting in the theory of terrestrial magnetism. I, therefore, resumed the inquiry and have been led to some novel facts which, to me, have appeared exceedingly important, by throwing a new light on the action of caloric on magnetism. They will be explained in the Third Memoir.

was reversed also. The new deflection was noted down, and the iron finally removed from the helix. The deflecting power of the current alone, when no iron nor magnet was in the helix, was also ascertained at different times during these experiments; two sets of which were made with two different batteries—the former by an old battery, and the latter by a new one. The results, with all the necessary particulars, are arranged in the following tables :—

First Series of Experiments.

Deflections with the magnet in the helix, with and without the electric current from the old battery: and magnet retouched.

With or without the Current.	Marked or unmarked end of the magnet nearest to the needle.	North or south end of the needle drawn towards the magnet.	Deflections.	
Without	Marked	South	15°	1
Ditto	Unmarked	North	16°	2
With	Marked	South	17°	3
Current reversed . .	ditto	ditto	7°	4
With	Unmarked	North	18°	5
Current reversed . .	ditto	ditto	9°	6
Magnet alone . . .	Unmarked	North	13°	7
Ditto	ditto	ditto	12°	8

118. The electro-magnetic force in the helix alone, by this battery, produced no perceptible deflection of the needle; but when the soft iron was placed in the helix, the mean of several deflections, with the currents in different directions, was 17°.

119. By taking the mean of the deflections 3 and 5 in the table, which are those obtained whilst the electro-magnetic action of the current conspired with that of the magnet, and comparing that mean (17·5°) with the mean of the deflections with the soft iron (17°), we find that they are nearly to the same extent. And by comparing these again with deflections 1 and 2, which are due to the magnet alone, we discover that a current which is incapable of exalting the original deflecting power of the magnet 2°, is yet capable of raising a deflecting power in soft iron, equal to the whole of that exhibited by the magnet, even when aided by the influence of the current. We discover also, by deflections 4 and 6, that the same current, when exerted in *opposition* to the energies of the magnet, is incapable of counteracting more than one-half the deflecting power of the latter. And we learn, by comparing deflections 7 and 8, which are those due to the magnet after being subjected to the *reverse* electro-magnetic action of the current, with deflections 1 and 2, that the *same* electric current, which excited so great a power in soft iron, was incapable of reducing the *permanent* action of the magnet more than one-fifth of that which it originally exhibited.

Second Series of Experiments.

Deflections with the magnet in the helix, with and without the electric current, with the new battery: and magnet retouched.

With or without the current.	Marked or unmarked end of the magnet nearest the needle.	North or south end of the needle drawn towards the magnet.	Deflections.	
Without	Marked	South	20°	1
Ditto	Unmarked	North	19°	2
With current . . .	Marked	South	25°	3
Ditto reversed . . .	Ditto	North	1°	4
Magnet alone . . .	Ditto	South	11°	5
Ditto	Unmarked	North	9°	6
Magnet re-magnetized.				
Without	Marked	South	21°	7
Ditto	Unmarked	North	21°	8
With current . . .	Unmarked	Ditto	27°	9
Ditto reversed . . .	Ditto	South	2°	10
Magnet alone . . .	Ditto	North	8°	11
Ditto.	Marked	South	10°	12

With this battery the soft iron gave a deflection of 18°; and the current alone, without either magnet or iron in the helix, about 1°.

120. In this second series of experiments there is displayed a manifest superiority of electro-magnetic action over that shown by the old battery; but although deflections 4 and 10, show that the electro-magnetic action completely counterbalanced the deflecting force of the steel-magnet, deflections 5, 6, 11, and 12, as obviously demonstrate that the original magnetic power was very far from being annihilated, and that, notwithstanding the vigorous electric current to which the bar had been subjected, the latter retained about one half of its original power, which that current was unable to subdue. Indeed it appears from both series of experiments that a great portion of the electro-magnetism of the helix operates merely on the *disposable* part of the magnet's force, and diverts it from its original direction, in the same manner as soft iron, or other magnets would do; and the electro-magnetic force thus engaged, is prevented from assisting the other portion in conferring permanent effects on the steel. When the constraining electro-magnetic force is removed, the liberated disposable force of the magnet with which the former had been engaged, again resumes its original direction, and gives the needle a new deflection, in the *same direction,* though not to the same extent as at first, (deflections 5, 6. 11, 12.)

121. I am not aware that any one would venture to assert that electric currents, more powerful than those employed in these experiments, still existed in the steel: and if not, to what cause are we to allude the retained magnetic force? There must be some agent in operation which still sustains the polar action, and resists the energies of the assailing electric current. That agent cannot be

electricity, or it would have been subdued by the counteraction of a superior electric force; it must, therefore, be admitted, that some other physical agent, perfectly distinct from the electric, presides over the polar forces of the steel magnet.

122. I am well aware that, had the electro-magnetic force of the current been more powerful, the magnetic forces of the steel would have suffered to greater extent; and it is possible that an electro-magnetic force might be employed of sufficient extent to completely annihilate the original polarity of the steel, or even reverse its polar action; but I should wish it to be understood, that to accomplish such an effect, the electric current employed must be very powerful indeed; and whatever extent of polarity might be exhibited by the steel after the removal of the exciting electro magnetic force, the *retention* of that polarity could not be supposed to depend upon that *absent* exciter, any more than the polarity of this, or any other piece of steel, could be supposed to be sustained by the absent magnet which first excited it: and our present knowledge of electro-dynamics does not permit us to indulge in the idea that any sustaining electric currents remain in the steel.

123. We have seen by the preceding experiments, that the power of the magnet was considerably lessened by the action of the electro-magnetic force in the helix; but it must be observed that the latter force had no *sustaining* power to contend with, excepting that exercised by the retention of the steel: but if the magnet be placed under the influence of a *sustaining* magnetic force during the time it is assailed by the electro-magnetism of the helix, it will be found that the latter is too impotent to make any other than a very slight permanent impression on the original power of the steel magnet; and, under some circumstances, not the slightest impression is accomplished. To prove this fact, I place the *marked* end of a magnetic bar, seventeen inches long, in contact with the *unmarked* end of the six inch cylindrical magnet whilst placed in the helix, the marked end of the latter being nearest to the needle, as represented by fig. 10. I now transmit the electric current through the helix, in a direction which tends to neutralize the magnetism of the inclosed bar. The current is continued for more than a minute, after which it is removed, and as speedily as possible, the long sustaining magnet is removed also. This done, the deflecting power of the cylindric magnet is again ascertained. The following table shows the results.

THIRD SERIES OF EXPERIMENTS.

Deflections with the magnet in the helix, with or without the Electric Current, from a new Battery.

With or without the electric current.	Marked or unmarked pole of the magnet nearest to the needle.	N. or S. end of the needle attracted.	Deflections.
Without the current . . . . Ditto	Unmarked Marked	North South	29° 31°
Ditto	Ditto sustaining magnet attached,	Ditto	65°
With the current tending to neutralize the magnet . .	Ditto	Ditto	59°
Current and sustaining magnet removed	Marked Unmarked	Ditto North	26° 24°

124. I next place the cylindrical magnet under the influence of two sustaining magnetic bars, each 17 inches long; submitting it, at the same time, to the action of an electric current, tending to neutralize it. The arrangement is represented by fig. 11, and the results were as follow:—

Fourth Series of Experiments.

	Mean Deflection of both Poles of the Needle.
Before the magnet was subjected to the action of the current	30°
After the magnet had been subjected to a current tending to neutralize it .	31°

125. When under the sustaining force of two magnets, we find that the electric current makes no impression on the small magnet on which it operated. The trifling power which the magnet gained during the experiment, was obviously due to the influence of the bars between which it was placed. The additional power given to the intervening magnet, by this means, is, however, but very small, never amounting to more than 2° of deflection, as I have ascertained by several experiments, by permitting the cylindrical magnet to remain between the poles of the two large ones, as in fig. 11, for two minutes in each experiment; which is a much longer time than it remained under the same influence after the removal of the electric current in the preceding experiments. Hence, since a sustaining magnetic force may be employed to any required extent, the obvious inference is this. *No electric current, however powerful, is capable of impairing the powers of a hard steel magnet, whilst the latter is under the protecting influence of a proper purely magnetic force.*

126. Having ascertained that the sustaining magnetic force does not operate as an exciting power (125), I was led to suppose that the power of the *protected* magnet is sustained by the mutual attractions of its own *disposable* forces (105, note) and those of the sustaining magnets; the north and south polar forces engaging with each other too intimately to be disunited by the assailing electro-magnetism in the helix. This view of the nature of the action led me to try soft iron as a means of sustaining the power of the magnet, whilst the latter was subjected to the action of an electric current, considering that a portion of the disposable force of the magnet would be employed by the iron, and thus be protected from the assailing electro-magnetic force: but it was found by the experiments about to be described, that soft iron affords no protection whatever to the magnet when assailed by a converse electro-magnetic force: but on the contrary, the iron facilitates the subduction of the original powers of the steel magnet.

127. The experiments were made by placing the cylindrical magnet in the helix, and ascertaining its deflecting power on the needle at the original distance of 12 inches. Then placing in contact with its remote pole a cylindrical bar of soft iron, 6 inches long and about an inch in diameter. An additional deflecting force is thus given to the magnet, which deflection is also noted down. Another bar of soft iron, $3\frac{3}{4}$ inches long, and about the same thickness as the former, was next placed in contact with that pole of the magnet nearest to the needle, and the new deflection thus given to the needle also noted down. This done, the electric current from a new battery was transmitted through the

helix, whose magnetic powers were opposed to the powers of the enclosed magnet. The following table shows the results:—

Fifth Series of Experiments.

Deflections with the magnet in the helix, with and without the soft iron and electric current from a new battery.

With or without the soft iron and electric current.	Marked or unmarked end nearest to the needle.	N. or S. end of needle attracted.	Deflections.	
Without current or iron . . .	Marked	South	30°	1
With the larger piece of iron . .	ditto	ditto	42°	2
With both pieces of iron . . .	ditto	ditto	65°	3
Do. with a converse electric current	ditto	North	40° then 19°	4
Current cut off, but iron remaining	ditto	South	25°	5
Magnet alone	ditto	ditto	10°	6

128. The principal circumstances to be noticed, in these experiments, are the singular changes of polarity by the soft iron, and the final subduction of a great portion of the force of the magnet. By deflection 4 we see a transposition of polarity by the action of the current. The new deflection thus given to the needle at first rose to 40°, but gradually sank down to 19°, where it remained permanent for some time. This reduction of the deflection was, of course, dependent on a reduction of polar energy in the nearest piece of iron: and as the polarity of the iron depended on the polar condition of the magnet, we learn that the transient transposition of its polarity is accomplished to the greatest extent, immediately after the current has got into full play, and that it gradually subsides for about one minute afterwards, at which time it has arrived at its minimum. These versatilities in the polar action of the magnet are observable in all cases when it is subjected to a converse electro-magnetic action, whether there be any iron attached to its poles or not, though without iron they are not so great as when that metal is present. They are exceedingly curious, and are involved in a theoretical principle, which it is not necessary to enter into at present. By comparing deflections 1 and 6 we find that the magnet has lost a considerable portion of its power, which portion is greater by 6° or 8° than that usually lost when no iron is present, all other circumstances being the same; which shows that the attachment of the iron to its poles facilitates the subduction of the original powers of the magnet. See also the first and second series of experiments.

129. I had next recourse to the reverse proces of that which was pursued in the last experiment. I placed the soft iron cylinder in the helix, and attached one pole of the cylindrical steel magnet to that extremity of it which was nearest to the needle; and whilst thus arranged, an electric current

was transmitted through the helix. The distance between the pivot of the needle and nearest pole of the magnet was 12 inches. The following results were obtained:—

Sixth Series of Experiments.

	Deflections.
Magnet alone, prior to being placed in the arrangement	38°
Magnet attached to the iron bar, the latter being under the influence of the current . . .	45°
Magnet alone, after the iron and current were removed	38°

The magnetism of the soft iron left no additional permanent power on the steel magnet.

130. Having ascertained that an electric current is capable of subduing a considerable portion of the original power of an unprotected steel magnet (119, 120), it became an inquiry of some interest to ascertain whether or not the same current, with the magnet reversed in the helix, was capable of restoring the power which it had previously subdued. For this purpose, the cylindrical steel magnet was retouched; and after its deflecting power, at the distance of eight inches, had been ascertained, it was subjected to the action of an electric current from a perfectly new battery, whose copper exposed about a square foot of surface, with a proportionate rolled zinc cylinder inside. The battery was made exceedingly active by a solution of nitro-sulphuric acid. The following table shews the results.

Seventh Series of Experiments.

Magnet alone,	previous to its exposure to the current	.	39°
Ditto	after being exposed to a *converse* current		21°
Ditto	after being exposed to a *direct* current		25°
Ditto	after a second exposure to ditto		.26°
Ditto	after several other exposures to ditto	.	26°

131. From this series of experiments, we learn that the active electric current here employed was incapable of restoring $\frac{1}{3}$ of that portion of the deflecting force, of a newly magnetized hard steel bar, which it was previously enabled to subdue, although as powerful during the one process as during the other. This exceedingly curious fact I have found in the results of several other experiments, and with batteries of different powers. But the same law does not hold good, unless the magnet has been magnetized to a high degree previously to its being subjected to the electric currents; nor, perhaps, will it be found *generally* exact, even under these circumstances, although I have not met with any results in direct contradiction to it. And although the ratio of the *subdued* and *restored* force may

vary, I have cause to believe that in no case will the restored force be more than one-half of that which had been subdued by the same current, when the magnet employed is hard cast steel, and not below the dimensions of that which I have described (115): and the voltaic plates of proportional magnitude.

132. Another interesting fact presented itself by neutralizing the cylindrical steel bar, and afterwards magnetizing it by the electro-magnetic action in the helix, whilst the latter was transmitting a copious and active current from the battery last described (129), furnished with a new zinc. The deflecting power which the steel acquires, by this process, is about one-half of that which it exhibits by means of ordinary magnetic excitation. I have doubled and trebled the coil in the helix, but in no case has the magnetic power of the steel increased above that I have just mentioned. The facts developed by these experiments, are partly attributable to the magnetic force receiving different forms of distribution by the magnetic and electric processes of excitement; though principally from an absolute incapacity in the latter of bringing forth those intense magnetic forces which hard steel is susceptible of displaying. There seems, indeed, to be a vigorous tension in the magnetism of hard steel, which that of electric currents cannot compete with in vanquishing those formidable resisting forces presented by hard ferruginous bodies, whilst undergoing the magnetizing process. Even the magnetism of soft iron, when brought into play by electric currents, though much more abundant in quantity, is of far lower tension than that of hard steel. This curious fact may be shown by experiments with two horse-shoe magnets; one of which shall be soft iron, brought into play by electric currents, and the other a permanent one of hard steel. When the cross pieces of both magnets are of soft iron, the iron magnet will have the greatest lifting power; but when both cross pieces are of hard steel, the steel magnet will have the greatest: and this is the case even when the power of the iron magnet (with soft iron cross pieces) exceeds the other to a considerable extent.

133. There is a remarkable phenomenon observed whilst magnetising hard steel by electric currents. The deflecting power of the steel is much greater whilst under the dominion of the current than after the latter is cut off. Now, as the helix alone exhibits no action on the needle (118, 119), the experiment shows that there is a temporary disposable force excited even in hard steel, which that metal does not exhibit when the exciting cause is removed. This fact probably arises from a new distribution, rather than from an absolute loss of the magnetism first excited by the current.

134. Having ascertained that the existence of electric currents is nowhere to be found in permanent steel magnets, (114) and also demonstrated the inadequacy of electric excitement to the production of that extent of magnetic energy in hard steel, which is susceptible of development by the ordinary process of magnetization (131), it may now be interesting to inquire how far the doctrine of *systems* of electric currents is susceptible of application in explaining the phenomena exhibited by permanent steel magnets.

135. Let N and N′, fig. 12. represent transverse sections of two cylindrical systems of electric currents, both of which are flowing in the same direction, as represented by the arrows: and let these cylinders be prolonged parallel to each other to any required distance behind the paper. Now, because of the electric currents on the adjacent sides of these cylinders running in opposite directions, in every pair of parallel sections, similar to those represented on the paper, those cylinders will exhibit a repulsion for each other throughout their whole length, or from end to end, according to the principles of electro-magnetism. Let, now, the remote extremity of the cylinder N′ be turned towards the spectator, permitting the cylinder N to remain unmolested. Under these circumstances, the *same*

extremities N, and N′, of the two cylinders whose adjacent currents, in the former case, flowed in *opposite directions,* will now flow in the *same direction,* as may be understood by looking at fig. 13: and consequently those extremities will attract each other. Again, let the arrows in fig. 14 represent the directions of two cylindrical systems of electric currents placed at right angles to each other, as C and C′. The adjacent portions of these currents flow in the same direction, and consequently will *attract* each other. Now place the electro-magnetic system C′ in either of the positions represented by fig. 15, and it is seen that the adjacent currents in C and C′ now flow in opposite directions, and will consequently *repel* each other.

136. From the above illustrations we learn that the extremities of two systems of electric currents will either attract or repel each other, according to the positions in which they are placed, and that they do not exhibit any specific polarity in the manner of ferruginous magnets, whose attractions and repulsions have no dependence whatever upon the positions in which their extremities are placed with respect to each other, but are invariably referrible to their specific polar character. There is, indeed, a striking distinction in the distribution of the magnetic force of steel bars, and that exhibited by electric conducting wires, whether the latter be in a simple strand, or coiled into any particular fashion. A conducting wire formed into a hollow helix displays but very little polarity exteriorly, in the direction of its axis, (118, 119,) because of the inner and outer sides of the coil exerting their magnetic forces in opposite directions: but with hollow steel magnets, the polar forces of each individual extremity conspire with each other, and operate in concert upon vicinal ferruginous matter, whether previously polarized or otherwise; and in precisely the same manner as such matter is operated on by *solid* magnets. Hence it is, that a polarized needle, or small bar, freely suspended, with its centre in the equatorial plane of a hollow steel magnet, whether *inside* or *outside* of the tube, will invariably assume one and the *same* direction: whereas a similarly suspended needle, with reference to, and under the influence of, a hollow system of electric currents, would assume *one* direction when *within,* and the opposite direction when *without,* the system: and as this peculiarity of magnetic arrangement would attend every system of electric currents that can possibly be formed, it is just to infer that the distribution of force displayed by steel magnets, or by loadstone, cannot be imitated by any system of electric currents whatever: and *vice versa,* the exquisitely uniform arrangements of enveloping magnetic action, so beautifully displayed around electric currents, appear to be totally inimitable by any known forms of ferruginous magnetic bodies.

137. It would be an almost endless task to examine every fact that might be brought to bear, directly, or indirectly, on the subject of this investigation. I have not dwelt on electro-magnetism to the extent I would have done, had my theoretical views on that department of electricity not been already before the public, although I have cited those electro-magnetic phenomena which appear to be the most important in the present discussion. In other departments of electricity I have enumerated such facts as have appeared necessary to collate with purely magnetic phenomena; and having discussed them individually as I have proceeded, a retrospection would be needless in this place. The inference to be drawn from the investigation of the *facts* alone, appears to me to admit neither of doubt nor equivocation; and may be thus briefly stated: *There are no facts on record which demonstrate an identity in electricity and magnetism; but, on the contrary, there are many phenomena which justify the idea of their being perfectly distinct powers of nature.*

ON A NEW COMPOUND

OF

CARBON AND HYDROGEN.

BY

MR. WILLIAM MAUGHAM,

Lecturer on Chemistry, at the Royal Gallery of Practical Science, Adelaide Street, London;
&c. &c.

FROM THE TRANSACTIONS OF THE LONDON ELECTRICAL SOCIETY.

LONDON:

PRINTED BY WILLIAM ANNAN, GRACECHURCH STREET.

1838.

VII. *On a new compound of Carbon and Hydrogen, by* MR. WILLIAM MAUGHAM, *Lecturer on Chemistry, at the Royal Gallery of Practical Science, Adelaide Street, London, &c. &c. Communicated in a letter, dated June* 21*st*, 1838, *addressed to the Honorary Treasurer of the London Electrical Society.*

Read 3d July, 1838.

Sir,

During the year, 1834, in consequence of certain experiments that were made with different kinds of artificial lights, at the Royal Gallery of Practical Science, by Messrs. Watkins, Gardner, Wilkinson, and myself, I was induced to pay considerable attention to the light produced by voltaic electricity, through the medium of charcoal points, for the purpose of rendering that light continuous and of the same intensity for any given period. I found that by increasing the number of the pairs of points, a corresponding number of lights might be accordingly produced of *apparently* the same intensity; the several pairs of points being placed within the voltaic circuit and insulated upon glass rods fixed in a piece of board. On bringing the points, thus arranged, in contact, and then separating each pair at the same instant (the apparatus being so contrived as to admit of this being done), sparks were produced simultaneously at the several interruptions. This experiment it is to be understood was suggested by my friend Mr. Williams, who very ingeniously contrived the necessary insulating apparatus alluded to. Whilst repeating this experiment with M. de la Rive, it was proposed by him that one of the pairs of points should be placed in water, when the spark, as might have been expected, was visible in that liquid, and at the same time a spark was visible at each of the other pairs of points out of the water. The experiment was continued for about a quarter of an hour, and as it was proceeding, a very peculiar odour, arising from the water in which the electrical light was produced, became perceptible.

On reflecting upon this afterwards, it struck me that some change must have taken place between the water and the charcoal during the passage of the electricity. To satisfy myself of the truth of this, I prepared a battery of about sixty pairs of four-inch plates arranged on Wollaston's principle; to the ends of the two electrodes I affixed two pieces of charcoal by means of platinum wires of sufficient length to allow the charcoal to be conveyed into a glass vessel containing distilled water. A glass tube was next filled with distilled water and then inverted in the water into which the charcoal points were to be immersed. On bringing these points together under the water so as to produce the electrical light with as little interruption as possible, the water underwent decomposition and the glass tube was soon filled with gas. When flame was applied to the mouth of the tube no explosion took place as when a mixture of oxygen and hydrogen gases is obtained by decomposing water when the action is electrolytic; but the gas burned silently with a flame similar in colour to that by which the flame of carbonic oxide is distinguished. Suspecting, therefore, carbonic oxide to have been produced, I passed 2 volumes of the gas obtained as above into an eudiometer with 1 volume of pure oxygen; the mixture was made over mercury, and lime water was then introduced above the mercury in the eudiometer. No change was observed in the lime water, but after passing an electric spark through the gases a gradual condensation of the whole took place and the lime water was rendered turbid, carbonic acid gas having evidently, in the first instance, been produced. If the spark be passed before the lime water is added the same condensation takes place as when we explode a mixture of carbonic oxide and oxygen, and lime water being then introduced it becomes turbid with a gradual

absorption of the whole of the gas. A *very little* residual gas was sometimes observed, which disappeared after the introduction of a crystal of proto-sulphate of iron, showing the residual gas to have been oxygen added in excess.

It is to be remarked that on passing lime water to the gas obtained as above described, I have observed that it is sometimes very slightly changed, *but this certainly does not always take place.* It would, therefore, appear that a trifling quantity of carbonic acid gas is occasionally formed along with the carbonic oxide. Neither hydrogen nor oxygen can be detected. Therefore should these gases be found by any one else along with the carbonic oxide, it will be owing to the action becoming electrolytic in consequence of the spark not being continuous during the operation, but when the spark is produced the action is not electrolytic, and it is only then when the carbonic oxide is formed.

When water, therefore, is acted upon as already described, carbonic oxide is produced by carbon combining with its oxygen, and at the same time a peculiar compound is formed by another portion of the carbon uniting with the hydrogen of the water, no hydrogen as has already been shown passing off in the gaseous state.

This compound, which does not appear to have been before observed, imparts a very peculiar and unpleasant odour to the water in which the charcoal points have been immersed, whilst under the influence of the battery, but the process should be carried on for about a quarter of an hour to be satisfactory, during which period the spark from the charcoal points should be continually visible under the water. The water thus impregnated with the new compound is of an oily nature, which may be seen by pouring it into a clean tube, then emptying the tube and holding it up to the light. When kept for some time the liquid loses its odour and there is a precipitate of carbon. This spontaneous change takes place whether it be exposed to the air or kept in a stoppered phial.

I formerly thought that the substance in question was a compound of 1 equivalent of hydrogen + 1 equivalent of carbon and proposed calling it *protohydruret* or *protohydroguret* of carbon, but we have no proof of its being so constituted; for although 1 equivalent of the oxygen of the water evidently combines with 1 equivalent of carbon to form 1 equivalent of carbonic oxide, it is not evident that the 1 equivalent of the hydrogen of the water also combines with 1 equivalent of carbon: it may combine with 2, 3, or more equivalents.

The production of light under water by means of charcoal connected with a voltaic arrangement is a well known experiment; but I have not met with any one who is aware that the above described change takes place, and therefore I venture to lay these observations before the public through the medium of the London Electrical Society.

When the experiment is made merely to obtain the compound under consideration in what may be considered a *concentrated* state, the less distilled water that is employed the better; about two or three ounces will be sufficient, it being merely requisite to keep the charcoal points covered with it, but more water will be required when it is intended to collect the carbonic oxide.

I have no doubt that some interesting results may be obtained by acting upon other fluids in a manner similar to the above with charcoal; and certain other substances might also be employed to effect the end I have in view; namely, the formation of compounds by producing the electric spark in water and other fluids, by causing the electrodes of a battery to be armed with different materials of a certain kind.

When steam is decomposed by passing it over red hot charcoal for the purpose of obtaining carbonic oxide, I do not find in the water over which the gas is collected anything like the compound in question. It may not be irrelevant to observe that Mr. Charles Cowper suggested to me that he

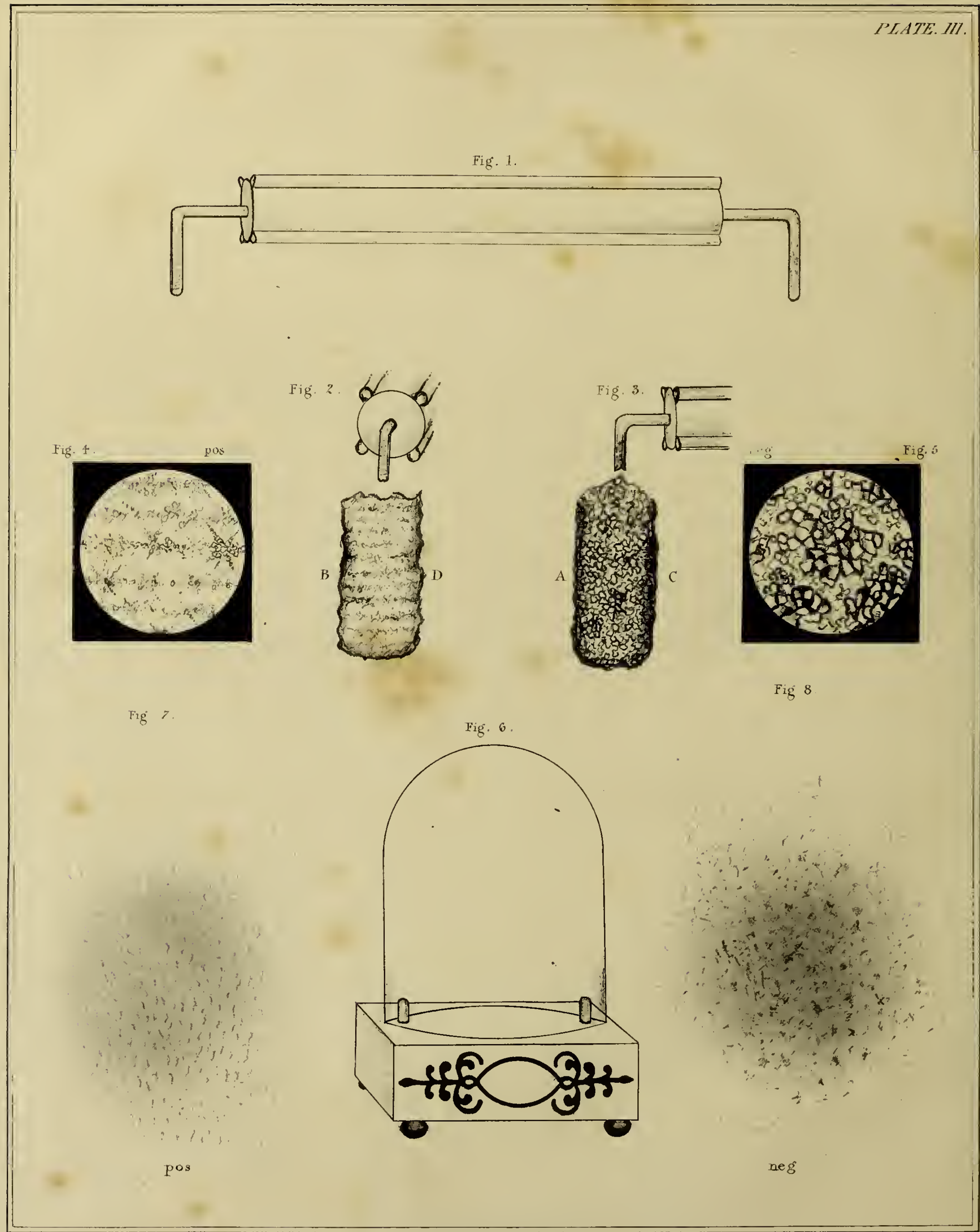
thought carbonic oxide might be substituted for hydrogen as a combustible with oxygen for obtaining intense heat. A considerable quantity was accordingly burnt with oxygen by means of my blowpipe, and when the jet of inflamed gas was thrown on a piece of lime, the light produced was very little inferior to that which is obtained when hydrogen and oxygen are employed for the same purpose, and probably the light would have been improved had the carbonic oxide been passed through lime to free it from the carbonic acid which it always contains.

May not the hydro-carbon which is the subject of this paper be identical with the oily compound noticed by Berzelius as being produced when hydrogen gas is obtained from iron and dilute sulphuric acid? (See *Lehrbuch* 186.)

I remain, Sir,
Your's truly,
WILLIAM MAUGHAM.

Royal Gallery of Practical Science,
Adelaide Street, West Strand,
June 21*st,* 1838.

Note. At the ordinary meeting of the Society, July 17th, the author of the preceding paper went through the experiments therein detailed, and the several results were highly satisfactory to the members present, being perfectly corroborative of what is stated respecting the formation of carbonic oxide and a previously unnoticed *hydro-carbon* when water is decomposed in the manner described.



Day & Haghe Lith^rs to the Queen.

OBSERVATIONS

OF NEW FACTS IN

VOLTAIC ELECTRICITY.

BY

MR. JOSEPH JEFFERY.

FROM THE TRANSACTIONS OF THE LONDON ELECTRICAL SOCIETY.

LONDON:

PRINTED BY WILLIAM ANNAN, GRACECHURCH STREET.

1838.

VI. *Observations of new facts in Voltaic Electricity, communicated by* Mr. Joseph Jeffery.

Read 19th June, 1838.

The present observer merely undertakes to describe and invite explanation of appearances, to which his attention has been recently directed by Mr. E. M. Clarke, of the Lowther Arcade.

The facts to be narrated may be generally defined as evidence of voltaic action, under circumstances, leaving, if not doubtful, at least not very manifest, the completion of a galvanic circuit. In styling the facts *new*, the narrator may be misled by the limited character of his own information; but their assumed novelty is rendered probable by considering, that they have resulted from means not at all likely to concur in the ordinary course of experimental investigation.

It is not customary with experimenters to allow batteries to remain for any considerable time excited, when out of use; but we have here to regard unplanned and unexpected results, obtained from a constant battery, the form and construction of which perhaps render it as uniform and unexceptionable an instrument in the determination of its own effects, as any kind of battery that could be devised.

The battery employed was the pile of Zamboni. Its elements are known to consist of tin-leaf, paper, and an oxide of manganese; the latter metal being laid upon one surface of the paper in the usual manner, after being formed into a paste with honey. And this pile is well known to resemble the column of De Luc in some of its effects, especially in causing divergence in the gold-leaf electroscope, by contact with either of its terminal points. The drawing, fig. 1, Plate III. is proportioned at half the size of the battery.

Some time ago Mr. Clarke combined a number of these piles by means of quicksilver connexions; for which purpose short terminal wires, bent at right angles, were attached to the extremities of the piles, and duly amalgamated. On concluding his experiment, the piles were severally folded in brown paper, as now produced before the Society. When unfolded, twelve months afterwards, the terminal wires exhibited the following results:—The amalgam had been decomposed, the copper appearing at the negative terminus in minute ruby crystals, upon a mass resembling a sulphuret, of varied colour, but chiefly the gold yellow; the mercury retaining its greyish colour at the positive terminus, at the base of more acutely angled and equally brilliant ruby crystals.

The crystals at the positive and negative wires were also of distinct arrangements, which the bent form of the wires will assist in describing. The negative wires were most uneven, from prominence of the crystals, on those sides, A and C, fig. 3, which may be regarded as the exterior and interior of the angle; but the intervening sides, B and D, fig. 2, of the positive wires, were of most uneven surface; while a lined arrangement of crystals, as represented in the magnified drawing, fig. 4, has been discriminated on their exterior and interior sides. Fig. 5 represents the magnified appearance of the crystals on the corresponding surfaces of the negative wires.

In seeking the course of action, the present observer was informed that the batteries, while enclosed in the papers, were kept in a dry situation; and certainly the papers themselves bear no signs of having been exposed to moisture sufficient materially to improve their conducting power. But with whatever allowance this circumstance may be regarded, it seems a question that may be worthily entertained, whether the electricity of a column, of a given intensity, and of a limited longitudinal extent, may not open for itself a path on any surface, however deficient in conducting power, provided

another condition be, as here fulfilled by the brown paper, that of a shelter from any considerable supply of atmospheric air.

It should be noticed that the elements of the pile are in the present instance enclosed in a plaster or cast of sulphur, and are thus insulated from any exterior portion of the current. By assuming that small particles of the sulphur may have been taken up by the external portion of the current, we should be able to account for the distinctive colour of the mass at the base of the crystals on the negative wires.

Attention has also been directed to results of a different character, obtained from another combination of these batteries. In this instance the connexions are of solder, and the combination terminates in pillars of brass, fixed in a stand, and closely sheltered by a glass dome, fig. 6. After being laid aside for a considerable time, there appeared on the brass pillars, and on the glass, on portions of its interior surface opposed to each other, and the nearest to the pillars, small collections of dust, apparently affecting some magnetic arrangement, differing at the positive and negative only in being more branched at the one than at the other. It is attempted to describe these appearances in the magnified drawings, figs. 7 and 8.

With a view to ascertain how far the completion of the circuit was essential to the activity of the pile, advantage was now taken of its form, to enclose it in a helix of copper wire, which was made to connect the terminal wires of the battery. In this state it exhibited slight magnetic indications; and if its small quantity of electricity had been economised by a better helix, it is probable that those indications would have been more decided.

While the terminal wires were unconnected, no such indications could be ascertained.

As to both these results, the one decidedly electro-chemical, and the other of an apparently magnetic character, the present observer resorts not to any system of Voltaism for an explanation, but expects that it will be found in the establishment of an electric circle under cover, and probably on the interior surface of a sheltering substance, in the one instance consisting of brown paper, in the other of the glass dome. He would, however, have great satisfaction in hearing the difficulties removed by explanations from other members of the Society, to whom, with this anticipation, the preceding facts are most respectfully communicated.

J. JEFFERY.

London, 18*th June,* 1838.

ACCOUNT OF SOME EXPERIMENTS

WITH A

SUSTAINING VOLTAIC BATTERY.

BY

ANDREW CROSSE, ESQ.

FROM THE TRANSACTIONS OF THE LONDON ELECTRICAL SOCIETY.

LONDON:

PRINTED BY WILLIAM ANNAN, GRACECHURCH STREET.

1838.

V. *An account of a series of daily observations, made by* Andrew Crosse, Esq., *of Broomfield, near Taunton, with a Sustaining Voltaic Battery, to ascertain the increase or diminution of the power of the same, as corresponding with the increase or diminution of the temperature of the atmosphere, during a part of the last winter, and commenced previously to the very severe frost which afterwards took place. Also a few remarks on the agency of heat in electro-crystallization.*

Read June 19th, 1838.

For upwards of two years past I have found it convenient in the formation of crystalline and other matters, by the electric agency, to make use of porous earthen pots, of the same nature as garden pots; but without an aperture in the bottom. One of these being filled with a compound fluid, A and B, and being plunged in a basin filled with another compound fluid, C and D; A of the one fluid having a greater chemical affinity, (as it is called) to C of the other fluid, than it has to B, with which it is united, and also B to D, so that by the admixture of the two fluids, double decomposition would take place; an electric current being passed by means of primary conductors proceeding from the poles of a voltaic battery in constant action, from one fluid to the other, through the pores of the pot employed, a slow union of A and C, or B and D, or both, takes place, either at the positive or negative pole, or on the inside or outside, or *within the substance* of the pot itself; or in more than one, or in all of these, according to the *nature* and *temperature* of the fluids employed, the *intensity* or *quantity* of the electric current, the thickness of the pots, and the presence or *absence of light*, which last is in most cases of greater or less importance, and in some absolutely essential. The result of this union is common to the production of regularly or irregularly formed crystalline matters more or less firmly adhering to the substance upon which or within which they are formed. I have used these pots in hundreds of experiments, in an infinite variety of applications, and with considerable success. I have likewise used them more or less extensively in the place of bladder in sustaining voltaic batteries, for which purpose they are admirably suited. They have, however, one defect. If, while sulphate of copper is used for the negative cells, a neutral salt be employed for the positive, in the course of time crystallizations are formed within the substances of the earthenware which separates the two fluids, and the pots are cracked in all sorts of forms—sometimes longitudinally; sometimes laterally; sometimes in concentric layers, the outer or inner portions scaling off like the bark of a tree; and sometimes in small angular or circular fragments which start off with a slight explosion, so that after some months' action the earthen vessel is spotted over with deep indentations either external or internal, or both. It is therefore safer and better on all accounts to avoid the use of neutral salts in the positive cells, which I commonly fill with simple water, when I wish to keep up a uniform action for a considerable time, and when I employ these pots merely in the place of bladder.

The following observations were made in a room exposed to the light, with a southern aspect, and situated about 800 feet above the level of the sea.—

Dec, 22. 1837. 10 P. M. I set in action a small sustaining battery composed of twelve two-inch square arcs of zinc and copper (the zinc not amalgamated), in small porous pots and glass basins; each zinc plate resting on a small piece of zinc, placed in a glass basin filled with common water; and each copper plate resting on a larger piece of copper placed within a pot which stood in the middle of the next glass basin, and which contained three ounces of sulphate of copper and water. It is a sim-

ple and economical mode of increasing the surface of the metals employed, to cause the pairs of plates to rest respectively on larger masses of the same metals, by which means small plates may be made to act in some degree with the power of large ones, and which partially saves the expense and trouble of casting. A Faraday's voltameter being filled with common pond or river water, was connected with the poles of the battery, and emptied of its gas each night at ten o'clock, and replaced in its former situation. A thermometer was likewise suspended above the battery, so that its bulb was immersed in one of the glass basins of water. This thermometer was examined at different intervals during the day and night, and the degrees of temperature carefully noted. It is obvious that by this arrangement only a portion of the electric fluid excited could possibly pass through the voltameter, as no acid was added to the water with which it was filled. For this I had reasons which I shall not here dwell upon.

The observations were continued for the space of one month or twenty-eight days, during which time neither water nor sulphate of copper was added to the cells of the battery, in consequence of which a good deal of the fluid had evaporated at the month's end. When the voltameter was first applied, a very small stream of the combined gases was extricated, but in the course of some hours it increased, and at the end of twenty-four hours 42-20ths or degrees of gas were evolved. It will be seen by inspecting the journal attached, that the battery did not arrive at its maximum of power till the third day from the commencement of its action. This was occasioned by the resistance of the earthen pot to the electric current, a thicker or a thinner pot affording a greater or a less resistance. It will likewise be seen that in general there was a more or less regular decrease of power, which seemed to be at the rate, as near as one may judge, of from one to two degrees of gas in twenty-four hours, supposing the temperature to remain the same; but that in general the power increased or diminished with the increase or diminution of temperature. Thus in the first week, as long as the thermometer stood at about 50, the diminution of gas was from one to two degrees in each day: but on the last day of the year when the thermometer had sunk from 50.5 to 47, the quantity of gas obtained was lessened from 61 to 57.5 degrees: also between the 6th. and 7th. of January, with a diminution of temperature of from 42.5 of the thermometer to 39, there was a diminution of gas of from 51 to 45 degrees. This is what one might more or less have expected; but I know not how an increase, and a somewhat considerable one, of the power of the battery could take place under a *diminution* of temperature. Thus, on January 13th, with the thermometer at 32, forty-five degrees of gas were produced; when on the preceding day, with the thermometer at 36, only 42 degrees of gas were liberated. Again, on the 17th. January, 47 degrees of gas were produced, with the thermometer at 34, when on the preceding day there were only 41 degrees of gas with the thermometer at 33. The one degree's increase of heat bears no proportion to the six degrees' increase of gas. Again, on the last day of the journal, with the thermometer at 32, there were 4 degrees of gas less than the preceding day with the thermometer at 31. This requires sifting and close examination. It may be observed that the degrees of gas produced on the last day, with the thermometer at 32, and with ice in all the cells, were exactly the same as on the first day with the thermometer at 50. It may also be noted that the total quantity of gas obtained in the fourth week was only 4.5 degrees less than that which was obtained in the third week, notwithstanding the natural diminution of power in the battery, the increased loss by evaporation of fluids, and the five degrees' diminution of temperatures. I was prevented from prolonging these observations by the freezing of the water in all the cells. I may here observe that I had, previously to these experiments, as I have since tried the effects of heat in combination with voltaic electricity in the formation of

crystals, that I have exposed various solutions under different conditions to the electric action, such solutions having been kept as nearly as possible at the boiling point, from one to six weeks, the apparatus being plunged in sand baths, with fires kept up day and night without a moment's intermission, and the solutions being constantly replaced as they evaporated. In sixteen of these experiments which were carried on at the same time, the evaporation was so great that it exceeded seven gallons in every twenty-four hours. I am not prepared at present to give a succinct account of the different results of these operations, but shall state generally the following conclusions.

1. A piece of yellow sulphuret of copper was exposed to the electric action, in sulphate of copper at the negative pole, *in the cold solution,* and found after a given time to gain a certain weight, the same being my friend Mr. Fox's experiment.

2. A similar piece of the same was exposed, exactly under the same circumstances, *in the hot solution* to the same electric power, and found to gain *thirty-one times* the weight of the preceding, within the same time. Such additional weight in both cases mostly consisted of metallic crystallized copper and red oxide of copper on the surface of each.

3. Although the solutions, in which the latter formation took place, were kept as constantly as possible at the boiling temperature, the crystals were generally of the most regular form, with their angles and facets quite as perfect as those of a natural formation.

4. In the production of crystallized copper and red oxide of copper, I found that with a single pair of plates plunged in boiling solutions the increase of crystallized matter averaged 60 *grains in each day,* or one ounce troy in every eight days; and that, consequently, even tons weight of crystallized copper and red oxide of copper may be formed in a comparatively very short space of time by an increase of electric power, and the quantity of solutions employed. The sizes of my plates were various, generally about two inches square.

5. By covering large plates of zinc, with plaster of Paris, unconnected with any copper plates, and laying them horizontally in large vessels filled with sulphate of copper, kept boiling, and well supplied with a fresh solution as the evaporation went on; the most perfect octohedral crystals of metallic copper and red oxide of copper, were formed to the amount of some ounces in weight in less than ten days. These crystals were fully equal in all respects to those formed by nature.

6. The same effects took place in the cold, but in an infinitely inferior degree. In this way I have formed crystallized copper, silver, and lead, upon a zinc ingot.

7. In breaking the thick earthen pans, in which some of these formations have taken place, crystals of various sorts are found within their substance. Also veins of metallic copper crossing them in various directions, very similar to what are termed the leaders to a metallic lode. Under some circumstances, perfectly insulated crystals of various sorts, not in connexion with either pole or with any metallic substance, are formed in abundance. This I have frequently observed, but in much less quantity, to have taken place in the cold, during the last two years. The above is a correct but rude sketch of some of the general results which I have met with in the combination of heat and electricity.

Query. May it not be possible to apply the combined action of a boiling heat and continued electricity, to the extraction of metals from their ores in a pure state, and with less trouble and expense than the plans now adopted?

ANDREW CROSSE.

London,
June 18*th,* 1838.

Extract from the Journal referred to in the preceding paper.

Day of the week,	Date.	Of gas. Degrees.	Therm.	Day of the week.	Date.	Of gas. Degrees	Therm.
	1837.				1838.		
Saturday	Dec. 23	42	50	Friday	Jan. 5	52	44
Sunday	24	64	50	Saturday	6	51	42½
Monday	25	69	50	Sunday	7	45	39
Tuesday	26	67½	50	Monday	8	43	37
Wednesday	27	65½	50	Tuesday	9	41	34
Thursday	28	64	50	Wednesday	10	44½	35
Friday	29	62	51	Thursday	11	44	34
Saturday	30	61	50½	Friday	12	42	36
Sunday	31	57½	47	Saturday	13	45	32
				Sunday	14	42	30½
	1838.			Monday	15	43	33
Monday	Jan. 1	56	47½	Tuesday	16	41	33
Tuesday	2	55½	46	Wednesday	17	47	34
Wednesday	3	54	44	Thursday	18	46	31
Thursday	4	52	42	Friday	19	42	32

Gas obtained.		*Average temperature.*		
1st week . . .	434°	1st week	a little above	50°
2d do. . . .	388°	2d do.	not quite	46°
3d do. . . .	310½°	3d do.	not quite	37°
4th do. . . .	306°	4th do.	a little above	32°

DESCRIPTION

OF A

GALVANOMETER,

BY WHICH THE DEFLECTING POWER OF AN ELECTRIC CURRENT HOWEVER COPIOUS MAY BE MEASURED.

BY

R. J. IREMONGER, ESQ.

FROM THE TRANSACTIONS OF THE LONDON ELECTRICAL SOCIETY.

LONDON:

PRINTED BY WILLIAM ANNAN, GRACECHURCH STREET.

1838.

IV. *Description of a Galvanometer by which the deflecting power of an Electric Current, however copious, may be measured. Communicated in a letter, dated April* 20, 1838, *addressed to the Secretary of the London Electrical Society.*

Read 5th May, 1838.

Sir,

I beg to call the attention of the Society to a modification of the Galvanometer, by which the deflecting power of any electric current, however copious, can be so reduced as to become measurable even by a delicate magnetic needle.

In this instrument, the electric currents, above and below the needle, are made to move in the same direction; in which case, their deviating effects being opposed, no motion will take place in the needle, as long as it is equidistant from both. But if the needle be not equidistant, then, naturally, the power of the nearest current will preponderate and cause immediate deviation; such deviation being the result of the difference between the two powers, and depending on the relative distances of the upper and lower wires from the needle. If then either of the reophores be made moveable, the effective deviating power of the other is entirely under the control of the experimenter; but, for the sake of calculation, the distances from the needle, at which the moveable current has one-third, one-half, two-thirds, &c. the power of the stationary one, must be graduated on the brass plate, which has been added for that purpose. By the use of a winder and multiplying wire the graduation is facilitated, as the influence of the multiplier is felt through a greater space than that of a single wire. I have also added a double wire for the sake of allowing uninterrupted passages to any electric current, however copious.

The method of making one reophore moveable by means of Spiral wires, was taken from an instrument most obligingly shown me this winter by Professor Majocchi, of Milan, to which I have added the arrangement of the wires, by means of which the reaction of the currents and consequent reduction of their deflecting powers is accomplished; and if the London Electrical Society judges the instrument worthy of a distinctive appellation, I beg to propose that it be called the Reacting Galvanometer.

I remain,

Sir,

Yours truly,

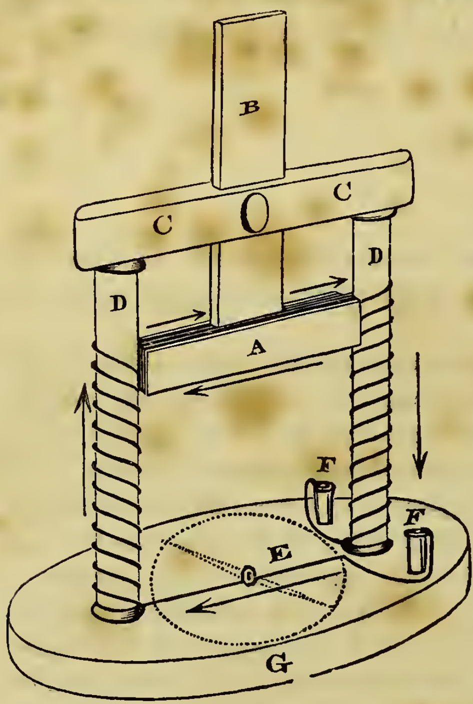
R. J. IREMONGER.

Bryanstone Street, Portman Square,
April 20*th*, 1838.

P. S. By using a winder of greater depth, an aperture may be made between the upper and lower coils so as to allow of the introduction of a needle; in which case the instrument becomes a common multiplier.

DESCRIPTION OF THE GALVANOMETER.

A. Winder carrying a coil and capable of being raised or lowered by means of the sliding piece B, which moves tightly through the cross bar C C.



D D. Columns supporting the cross bar and fixed at their bases to the sole, G. These have the termination of the coil round them in spirals.

E. The end of the spiral on column D, let into a groove in the sole.

F F. Mercurial cups.

The situation of the magnetic needle and graduated card is shown by the dotted line and circle; and the direction of the wire on the columns and winder, by the arrows.

It will be perceived that since any current circulating through the wires moves in the same direction in the lower part of the coil above the needle, and in the terminating wire beneath the same, the coil will exercise a counteracting effect on the needle dependant on the space between them, and therefore that the deviations will be produced by the excess of power in the nearest or under current.